THERMAL DISCRIMINATION AND WEBER'S LAW

With a theory on the nature and function of sensory adaptation

BY

ELMER A. K. CULLER

Department of Psychology
University of Illinois

ARCHIVES OF PSYCHOLOGY

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We here present, in a thoroughly revised and greatly extended monograph, the empirical findings incorporated in a study (unpublished) for the Psychological Laboratory of the University of Chicago; together with the inferences and theory which, in our opinion, this evidence justifies. We are indebted to James Rowland Angell and Harvey Carr for aid and counsel in a variety of forms; to various members of the Psychology department of Ohio State University for help and facilities in the course of experimentation; and to Madison Bentley, for useful criticism of the completed manuscript. Observers and other assistants will be mentioned at appropriate places in the text.

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Thermal Discrimination and Weber's Law

CHAPTER I.

Introduction

1. Statement of Problem

The purpose of this investigation was to make a careful survey of temperature-discrimination within a range not involving pain or extreme discomfort (16° to 48° Centigrade), with especial reference to the validity of Weber's law; and to note any other matters of psychological interest which might appear in that connection.[1]

[1] The term "limen", as we use it, is defined with care in the sections having to do with procedure and statistical analysis of the data; see especially pp. 48 and 54. By "Weber's law" we mean nothing more nor less than the relation expressed in the formula, $y = a + bx$, where y equals a given limen; a is a constant whose magnitude is fixed by the value of y when x equals zero; x is the size or value of the standard stimulus, while b, finally, is the constant (Weber) ratio of y to x, such that an increment of Δ in x necessitates an increment of $b\Delta$ in the correlative limen, y. These two concepts are often beset with questionable hypotheses, which we purpose to avoid by simple and objective definition.

2. Abstract of Procedure, Results and Conclusions

To aid the reader in gaining a quick and comprehensive orientation, we propose, in lieu of the usual "summary" at the close of a monograph, to give a brief abstract here of the important facts which the present investigation has yielded, and the inferences and theory we believe these facts to justify.

The historical sketch (Chapter II) is followed by a description of the apparatus (Chapter III) with special reference to the constancy and reliability of the stimulus-controls. Chapter IV gives a critical presentation of method and attempts, by an extensive array of evidence, to justify every detail of procedure, especially insofar as it departs from customary practise. In Chapter V is given a rigorous derivation of Urban's formula, and a number of features not commonly noted are emphasized. In Chapters VI and VII are presented our facts, inductions and theoretical proposals. Chapter VIII is occupied with tables of data and bibliography.

The apparatus consisted of seven jars filled with water and conveniently arranged on a table. Adequate heating, cooling and stirring devices were used; the thermostatic controls held the water indefinitely within less than .01°, in the mean, of its stated temperature. The first problem in quantitative work (control of stimulus) was therefore solved with a precision and reliability heretofore unattempted for psychological work in the thermal field.

The following temperatures (Centigrade) were covered in the order shown:

Group I: 32, 28, 24, 20, 16, 20, 24, 28.
Group II: 48, 44, 40, 36, 32, 28.

The writer did most of the observing himself; a variety of evidence shows that the results were in no way prejudiced thereby. The conditions of the experiment made it necessary to compare the same stimuli repeatedly in such wise that O. could remember how he judged the same combination before; an elaborate analysis of the figures revealed no appreciable effect from this procedure.

Preliminary data were secured from several observers by the method of limits, the principal data from the writer by several forms of the constant-stimulus method. At the beginning of each series of judgments was an adaptation-period of 3-5 minutes. In adaptation and in the process of judging, the four fingers of each hand were immersed in the water, its surface coming just below the body of the hand. The movement of the hands, when comparing two stimuli, was regulated with a metronome; the time of exposure to each stimulus was two seconds, and of exposure to the air, in passing from one to the other, was likewise two seconds. Six jars were used, each one in turn serving for standard and comparison; fifteen combinations in each time-order were thus provided, all of which were compared four times in one series, making 120 judgments in all.

The data were treated by Urban's modification of the psychometric function $\phi(\gamma)$. A critical derivation of this function is given. The p.e.'s of the limens are determined neither by Urban's nor by Thomson's formula, but directly from h, whence we have the s.d. of the Gaussian integral curve which is fitted to the observed percentages.

We now proceed to note briefly our principal findings, inferences and theoretical proposals.

a. Procedure

(1) By careful analysis of returns, we show that the director of a quantitative investigation can secure the benefits of extensive observation in his own problem without in any wise impairing the reliability of his results (p. 22).

(2) The advantage of comparing the smaller stimulus-differences more often than the larger ones is exhibited (p. 28).

(3) A mode of obviating errors, which may arise from the influence of a given judgment upon judgments immediately following, is demonstrated (p. 32).

(4) Repeated comparison of the same (within the stated limits of variation) stimuli in close succession, even when observer knows full well how he judged them before, is shown to have no appreciable effect of any sort upon later responses, that is, upon the reliability of the data as a whole (p. 33).

(5) The procedure of using each constant as both standard and comparison-stimulus in turn is shown to be not merely feasible but highly advantageous (p. 28).

(6) The real problem of instruction in discriminative work, viz, differentiating true (thermal) from specious (tactual and pain) criteria of judgment, is analyzed (p. 85).

(7) "Doubtful" is accepted as a better intermediate judgment than "equal", for the reasons given (p. 96).

b. Statistical Treatment

(8) A simple method of calculating the probable error of the limen is derived, which gives a value apparently as reliable as Thomson's and can be computed in about 1% of the time needed for his (p. 52).

c. Experimental Results: Facts and Inferences

i. On Adaptation

(9) Complete adaptation (thermal indifference) is found to be rarely, if ever, attainable, save in the region of skin-temperature; even when temporarily achieved, it cannot, as a rule, be indefinitely retained (p. 40).

(10) The time required for adapting to a given temperature-level (i) has a direct and almost linear relation to its departure from skin-temperature; (ii) varies directly with

the absolute limen (minimum perceptibile) at that level (p. 105).

(11) The degree of possible adaptation (the 'adaptive index') at a given temperature-level (i) varies inversely with its departure from skin-temperature; (ii) varies inversely with the absolute limen (minimum perceptibile) at the same level (p. 107).

(12) Cold is found to be less adaptable (that is, less amenable to a change of adaptive conditions) than is warm (pp. 82, 102, 113).

(13) When a receptor is adapted to paradoxical cold it is much less affected by normal cold stimulation as well (p. 65).

ii. On Thermal Discrimination and Weber's Law

(14) The relation known as Weber's law is proved to be a function of adaptation; it holds clearly and consistently for absolute limens (minima perceptibilia) but not at all for differential ones (minima distingibilia (pp. 72, 101).

(15) With absolute limens (receptor being always adapted to temperature of standard; method of constant stimuli), the *Weber ratios* for warm and cold limens from 16° to 44° are found to range between .0035 and .0053. These are the lowest fractions ever reported for any sense, but have high reliability for several reasons: (i) they are signally confirmed by other observers using another method; (ii) there are no more than 7 chances in 100,000 that the combined limens would, by chance, continue to enlarge on each side of their minimum at 28° by the increments actually found; (iii) the adjusted limens deviate from the observed in no case more than .0006° (pp. 60f., 76).

(16) With differential limina (receptor being always adapted to skin-temperature, 32°), no approach to a Weber constant ever appears; on the contrary, the ratio decreases rapidly as we depart from 32°. Instead of the rectilinear increases of (15) we find a strongly negative curvature in every case. The reality of the difference is abundantly confirmed by data from all observers and by various methods (pp. 72, 77, 80).

(17) The differential greatly exceed the corresponding absolute limina; their magnitude and their p.e.'s both prove how uncertain discrimination becomes at a temperature wide-

ly differing from that to which the sensory surface is adapted (pp. 72, 79).

(18) Optimal discrimination is found near 28°, where the hand can accurately appreciate differences of three to five hundredths degree (p. 60).

(19) The limens at the absolute minimum (28°) are relatively too large; at the extremes there is a little evidence that they are proportionably too small (A, 2: 16° and 44°). The latter statement awaits confirmation; if true, it would suggest that the absolute limens constitute an ogive instead of a straight line—an important hypothesis (pp. 61, 70).

(20) The sensitivity of the left hand (in the case of the principal observer) proves to be more than twice as great as that for the right hand, at skin temperature; we thus find a wide difference in functional efficiency between (normal) homologous areas of the same organism (p. 71).

(21) There is some evidence that greater sensitivity of one hand for warm does not of necessity imply equally greater responsiveness to cold and pain (p. 84).

(22) Upon passing through the air from one stimulus to another, cooling disturbs the cold limen, and warming the warm one, less in the more susceptible hand than in the other (p. 71).

(23) Likewise, cooling affects the cold limen more than the warm, and warming alters the warm limen more than the cold (p. 65).

(24) Time-errors are induced when the hands are warmed or cooled in passing through the air; space-errors are sometimes caused by the unequal sensibility of the hands; both are eliminated by our mode of handling the data (pp. 64, 70).

(25) We show that Müller's general and special "judgment-tendencies", by the use of our procedure, can be and are abolished; neither has an effect of more than two or three per cent (p. 99f.).

(26) The nature, locus and temporal course of true thermal criteria are analyzed (p. 91).

(27) The intrusion of intensive pain has a variant effect upon thermal discrimination with different observers; with some the limen is much increased, with others much reduced (p. 78).

iii. On General Psychophysical Methodology

(28) The point Xi, where the tendencies to judge cooler and warmer precisely balance, where both responses are equally probable, is shown to be extraordinarily useful: (i) as the subjective boundary dividing Warm and Cold, i.e. the zero whence the true limens are measured; (ii) in revealing and removing an error of expectation or habituation, which could be detected and measured in no other way. Methods commonly employed to this end (distributing stimuli symmetrically about the limen) are quite inadequate (pp. 57, 32, 62).

(29) In addition to the traditional time-space errors of Fechner, *p* and *q*, we show that a third (here called *m*) arises, when the hand moves twice to the right in passing from first to second stimulus and twice to the left. Although wholly ignored in the classic treatments of Müller, Titchener and others, *m* has precisely the same logical position and theoretical import as *p* and *q* (p. 63).

(30) We exhibit and rectify an error in Müller's treatment of the "judgment-tendencies" (p. 98).

(31) In the method of limits, the four observers divide sharply on an error of 'expectation', two making the 'expected' change relatively early, the other two relatively late. The latter two are the more competent and careful observers (p. 84).

d. Theoretical

(32) A theory of thermal sense-function is developed in terms of *adaptation* (p. 113):

i. The absolute limen is an inverse measure of the organism's adaptive capacity on a given level.

ii. The differential limen is a direct measure of the rate at which adaptation proceeds.

iii. The more rapidly and completely adaptable a receptive mechanism is, the *smaller* its absolute limen and the *larger* its differential limen.

iv. The "intensity" of any thermal experience is therefore a direct measure of the rate at which the organism is accommodating to the stimulus.

v. Any form of sense-experience is a signal that the organism is, for the time, ill-adapted or mal-adjusted to its environing situation. Slow and imperfect adaptability, as in the case of pain, shows that the individual is being stimulated with forms of energy which he is not prepared to meet; easy and high adaptability, on the contrary, proves that he is well equipped to cope with the modes of stimulation to which he is being exposed.

CHAPTER II.

Historical Sketch

E. H. Weber (*29*, 98) dipped the same finger or hand alternately into two jars of water standing side by side. He found that, the whole hand being immersed each time, he could distinguish one-fifth to one-sixth degree, Reaumur. "Most" people, he says, can distinguish with certainty a difference of 2/5° R. He did not find greater differences necessary to distinguish between two temperatures around 14° R. (= 17.5° C.) than in the region of blood-temperature (about 37° C.).

Fechner (*11*, I, 201 ff.) dipped the first two fingers of the right hand into water up to the second joint; alternating between the two jars until the judgment was formulated. He found that, from 19° to 31° R. (that is, about 24° to 39° C.), the just noticeable differences conformed to Weber's law "sehr wohl", if the magnitude of the stimulus was taken to be the amount by which it exceeds 14.77° R. (the approximate mean between freezing-point and blood-temperature); the Weber ratio for this range he finds to be .03623, which gives a fair agreement between observed and adjusted values, but greatly exceeds the ratio found in our study (.004 to .005). He states, in brief, that from 10° R. downward the just noticeable differences increase far more rapidly than is conformable with Weber's law; from 10° to about 19° R. the j.n.d.'s are too minute to be accurately measured with his thermometer; from 19° to 31° R. they agree very well with the law, as noted above; from 31° R. upward, he reports no observations but thinks it "wahrscheinlich" that the j.n.d.'s again increase too rapidly to conform with the law, as they did below 10° R.

Lindemann (*14*) dipped the right hand, to the wrist, alternately into two jars of water, the one being constant, the other being adjusted to apparent equality with the first (method of average error). Within the range 26° to 38° C. in one series, and 31° to 43° C. in another, the predominant error was + .05°; above and below these limits the error went as high as .5° to .8°. The number of trials was small, the experimental procedure was apparently very crude, and one

may wonder by what means the error was measured to one-hundredth degree C.

Alsberg (*3*) dipped the index finger, up to the first joint, into one glass of water, kept it there for 10 to 20 seconds, then transferred it to a second glass. His results show little regularity, discrimination reaching its maximum at 35°.

Nothnagel's (*20*, 284) method was the same as Fechner's above, except that one finger was used instead of two. He found most accurate discrimination between 27° and 33° C.; slightly less accurate from 33° to 39° and also from 27° to 14°; from 39° to 49° and from 14° to 7° sensitivity decreases rapidly. He does not give the results in detail.

Müller (*17*, 223) communicates no new observations but concludes, after surveying the discordant results of those previously reported, that the validity of Weber's law in this field is not established; while Thunberg, in 1904, closes his treatment of thermal discrimination with the words: "Wie diese Darstellung zeigt, sind die bisherigen Untersuchungen über die Unterschiedsempfindlichkeit für Temperaturreize spärlich, und die meisten Fragen, z.B. über die Gültigkeit des Weberschen Gesetzes, über die Bedeutung der Adaptation usw., bleiben noch zu lösen" (*19*, III, 688).

Abbott, in her study on adaptation and the difference-limen for temperature (1914), reports one curve which, in her opinion, applies to our problem. This curve "is not in accordance with the Weber Fechner law but it is only one curve for only one subject and no conclusions can be drawn on such slight evidence." (*1*, 36).

A recent study by Pütter (21a) contains material of interest which we hope to make use of elsewhere in greater detail. He finds that the least perceptible temperature-differences, when applied to the ball of the thumb, never fall below .5° to .6° C.; that sensitivity is most acute in the region of 28°; that thermal discrimination accords not at all with Weber's law but instead with a "new" threshold-law (formulated by Pütter himself) which states that the differential limen becomes *proportionably larger* as we depart from skin-temperature in either direction. Of these findings only one agrees with our own: that the organism is most responsive to thermal impressions in the vicinity of 28°.

Some studies on adaptation will be used in the body of the text; while other investigations dealing with the relative sen-

sitivity of different parts of the body, and the like, need not delay us here. Of all the observations reported on thermal discrimination, those of Nothnagel are the most comprehensive and agree most fully with our own findings.

It is by no means a coincidence that the field of temperature-discrimination was but little worked during a period when the facts and methods of psychophysics were being investigated and threshed over in detail. The special difficulties connected with adequate control of the stimuli, with adaptation, and the like, were recognized by Fechner and Müller; temperature thus became, logically, one of the last fields to be surveyed, after methods had been developed and refined in other senses more easily approached. In attacking this problem, the writer hoped, by the use of more delicate methods of temperature regulation than had been employed heretofore, by securing extensive collections of data from the same observer, by the use of complete series (Vollreihen) in the method of constant stimulus-differences, and by treating his data with the more refined statistical methods of modern psycho-physics, to achieve something like a definitive solution of the problem for the range of temperatures, and by the types of method, here employed.

CHAPTER III.

Description of Apparatus

1. *General Arrangement*

The apparatus in its final form, as used for the method of constant stimulus-differences, consisted of seven jars suitably arranged on a table. They were at a convenient level for O (the observer), when standing, to move his hands readily about and immerse them in any desired jar. Each was covered with a lid; this was provided with a large opening at one side, through which O could immerge his fingers in the water, the surface of which was two to four cm. from the top of the jar. Extending downward from the lid and supported by it were two heating units, an agitator and a thermostat.

2. *Heating and Agitating Devices*

Both of the heating units were carbon-filament electric bulbs. One was a variable heater, which went "on" and "off" in rapid alternation, as controlled by the thermostat; the other was "on" continuously and had two functions: a) to keep the temperature of the water always a few degrees below the point for which the regulator was set. A small variable heater could then be used for the remaining two or three degrees; the lag of the thermostat and resultant oscillation of the temperature were thereby much reduced. The second function of the continuous heater was b) to keep the interior of the jar light at all times. In this way O could properly guide his hand in the process of immersion—a completely dark jar was found to be inconvenient in that respect.

The agitator or stirrer was of a simple screw-propeller type, having three blades so adjusted as to give constant movement of the water, without causing superficial waves or strong currents. It is obvious that more heat will be exchanged between water and hand if the water streams rapidly over the fingers than if it moves slowly. It was, therefore, necessary to have the stirring as nearly equal in all jars as possible. To secure approximately equal movement in each jar, 1) the stirrers

were of the same size; 2) the curvature of the flanges was carefully adjusted; 3) they had the same position in each jar; and 4) they were all driven at the same rate. Frequent observation with both eye and hand revealed no apparent, or at least no gross, differences in the rate of flow of the water. That the stirring fully served its function of keeping the temperature of the water uniform throughout the vessel was shown in repeated tests with a Beckmann thermometer; which revealed differences in no case exceeding .005° between the upper three cm. of water and that at the bottom of the jar. The stirrers were turned by means of belts passing over a steel shaft fixed on the rear of the table and run by a motor.

3. *Thermoregulators*

The thermostatic device was similar to those in common use, but was designed, after numerous trials and modifications, specially for this work.[1] The form finally adopted consists of two upright tubes, filled with toluene[2] and supported from beneath by a central stalk midway between them; this in turn was full of mercury and was sealed at the top to a circular metal piece, which was fastened to the supporting lid. A glass capillary above the lid was given a threaded base, which screwed into this head-piece and could thus be adjusted at will. An electro-magnetic relay, which controlled the variable heater, was operated in the usual way by the "make" and "break" of the circuit as the mercury rose or fell in the capillary. The meniscus was kept clean and dry and the smallest practicable amount of current (.03 to .05 ampere) was used so as to reduce the break-spark, and consequent oxidation of mercury at the contact-point, to a minimum. As a consequence of these and other measures, the contact made and broke some ten to twenty times a minute throughout the working-period. The continued snapping of the relays, in fact, resembled the clicking of a battery of telegraph keys. The resultant fluctuations, as measured by repeated tests at various times and tem-

[1] The writer wishes to acknowledge the assistance of Dr. Marion Hollingworth in solving certain technical difficulties connected with making these instruments.

[2] A filler for thermostats should have four properties: large coefficient of expansion, small heat capacity, slight compressibility, high thermal conductivity. No substance has all these properties in ideal measure but toluene combines them to relatively high degree. Cf. Ostwald-Luther (21, 113).

peratures, were less than $\pm.005°$[3]; even though the temperature of the jars might rise or fall slightly within an afternoon, the periodic oscillation, in ordinary conditions, did not exceed the above figure.[4] To be sure, as anyone familiar with such work knows, sensitive instruments, when made and operated under conditions far from ideal, are not free of defects and difficulties. On some days, particular thermostats showed more lag than usual; the cause could not always be at once detected; or a series could not be stopped to remedy the difficulty; so that, a few times, the fluctuation reached as much as .03° or even .05° on each side of the stated value. It must be understood, however, that this degree of latitude was distinctly rare. Resorting to estimate,[5] the writer considers the statement conservative that the maximal oscillation in 50% of the cases used in the data herewith presented did not exceed .005°, and in 90% of the cases did not exceed .025°, on each side of the stated value; so that a temperature marked 36.05° would, in about half the cases, range from 36.045° to 36.055°, and in only a small fraction of cases would range beyond 36.025° and 36.075°.

The temperatures were taken and recorded before and after the experiments of each day. In the later stages, additional checks were often made during the working-period; but the initial and final readings were invariably secured. In many cases the final reading showed a rise or fall of several hun-

[3] All temperatures are in centigrade or celsius units.

[4] The reader will clearly distinguish the regular make-and-break oscillations, due to lag in the thermostat, from the gradual rise or fall over a period of some hours, due to various causes presented below (p. 18); the two are independent phenomena.

[5] The writer needs no reminder that estimates are precarious; his statement above, however, is far from a mere guess. It rests upon:

a) His own observations with the Beckmann, above alluded to, of the actual deviations both when the regulators were working precisely (with rapid sequence of "on" and "off") and also when their "rhythm" was slower; in this way he learned to appraise, with some accuracy, the range in temperature from the rate of alternation of the variable lamp. By means of this direct index, therefore, the latitude of change could be, and was, noted every hour of every working-period; any slowing of the normal "rhythm" was at once apparent; the cause could be sought and remedied, either between series or at the close of the day.

b) His repeated inspection of the mercury contacts—once per hour during the working-period. This constant attention also helped to reveal any failure of the regulators at their critical points.

Every effort was thus made to keep the apparatus always at maximal efficiency; and it is primarily upon this personal scrutiny of the instruments every working hour of the period covered by these observations, that the estimates above are based.

dredths, sometimes more, apart from the oscillations above discussed. By far the commonest cause of these changes was oxidation of the mercury at the contact. This would normally bring on a slow rise during the day; but occasionally the oxide collected on the inside of the capillary and, being a conductor, closed the circuit before mercury and wire were in actual contact, thus causing a slight drop. Inasmuch as these two causes were presumably gradual in their effect and therefore justify linear interpolation between the temperatures at beginning and close of the interval; and inasmuch as no values appear in the data actually used, if their discrepancy violates the carefully defined limits given below (p. 116), we believe these factors introduce no appreciable error into our results.

To put the matter into quantitative form, Table I was compiled with the design of giving, for the whole body of data

TABLE I

AVERAGE STIMULUS-CHANGE PER HOUR IN TEN-THOUSANDTHS DEGREE

		Decrease	*Increase*	*No change*	*Combined values*
Group I	Mean	55	80		57
	Median	40	60		33
	Number of intervals	395	730	274	1,399
Group II	Mean	122	135		118
	Median	67	100		86
	Number of intervals	155	163	30	348

Distribution of the combined values

	0 to 49	50 to 99	100 to 149	150 to 199	200 to 249	250 to 299	300 to 349	350 and over	Total
Group I	791	306	169	75	23	21	12	2	1,399
Group II	105	72	70	23	33	14	14	17	348

How to read the table: in 395 cases of group I, we find that the temperature fell from one reading to the next; the mean decrease per hour for these intervals was .0055°, the median being .0040°. Under "Distribution" it appears that, in 791 intervals of group I, the change per hour lay between .0000° and .0049°.

herein presented, an exact summary of the average rise or fall per hour between successive readings. Thus, if a jar reads 32.10° at 12 m. and 32.13° at 5 p.m., the mean rise per hour is .006°. It will be seen that the distribution for each group,[6] especially the first, is of the J-form, corresponding by inspec-

[6] The experimental work fell naturally into groups I and II, the former covering the temperatures from 32° down, the latter from 32° up.

tion to Pearson's type III; cf. Elderton (*10,* 65). The medians are therefore markedly smaller than the means. It now appears that, in group I, the mean change (rise or fall) per hour, during 1399 intervals, was .0057°, the median being .0033°. In a normal working-period of four hours, the mean change in level would thus be about .02°. If the values are distributed, we see that 791 of the 1399 (57%) are less than .005° while 1097 (78%) are less than .01°. These figures show, more clearly than many words, the constancy of the regulators.

It may be noted that the values in group II are about twice as large as in group I (the combined means being, for example, 118 and 57, the medians 86 and 33). Does this fact mean that the reliability of the thermostats decreased during the second phase of the work? In small measure, this may be true; it seemed a little harder to keep the regulators at maximal efficiency in the warm temperatures than earlier. The writer has no figures to support this impression, which may have arisen partly because an assistant at the time was less competent than his predecessor had been. Be that as it may, the decrease in reliability, when scrutinized, seems to be largely an artifact. In group I only two readings a day were made—before and after the working-period; in II additional checks were often taken during the period. To be exact: in I, extra readings were taken on 2 days of 51 (4%); in II, on 17 days of 30 (57%); a ratio of 14 to 1. As a result, the mean interval between successive readings in I was 5.1 hours; in II was 2.8 hours—about half as long. These repeated checks served to increase artificially the apparent change per hour. To illustrate: given a jar oscillating between 40.10° and 40.11°; at 1 p.m. it reads 40.10°, at 5 p.m. 40.11°; mean change per hour is .0025°. If two more readings are intercalated, we may get: 40.10, 40.11, 40.10, 40.11; mean change per hour is .0075°—just three times as much as before. Hence, the more readings, the greater the apparent change—an effect due merely to oscillation, not to actual change of level.

The temperature of each jar at any given hour was regularly determined by linear interpolation between the readings next before and after. It is therefore of some consequence to know the mean change per mean interval, in order to fix the range by which the interpolated value is likely to depart from the actual temperature at the time. In Table I the mean change per hour for group I is .0057°, for group II is .0118°;

the mean interval between successive checks in I is 5.1 hours, in II is 2.8 hours; the mean change per mean interval, therefore, is .029° in I and .033° in II. In view of the continued scrutiny to which the regulators were subject, it seems conservative to assume that, if a jar rises .029° in 5.1 hours, it has not appreciably (that is, beyond the amount of its regular oscillation) ranged outside those limits during the interval; whence it follows that the mean maximal discrepancy between interpolated and true values during the time will be just one-half the total rise. In the case of the two means above the figures would be .0145° and .0165°; in other words, the *maximal* error possible from interpolation in both groups is, on the average, about 1.5 hundredths. In view of these facts, the policy of fixing temperature-values by linear interpolation, when necessary, seems entirely justified.

Despite repeated difficulties, therefore, in achieving and maintaining the desired degree of precision in the regulators, the writer believes he was enabled to attain an accuracy of measurement and control which, while by no means the finest attainable, was at least adequate to the demands of our problem. One cannot, certainly, without a physical equipment far beyond the resources of the ordinary psychological laboratory, hope to gain a degree of stability and ease of control in temperature-constants such as can be got with relative ease, for example, in the case of metal weights;[7] but at least the variation of the constants should be only a fraction of the observer's limen, if reliable results are to be possible. Inasmuch as preliminary trials revealed a threshold considerably less than .10° under optimal conditions, it is clear that a variation of .10°—the limit of accuracy in most commercial thermo-regulators—is quite too large.

4. *Cooling Devices*

Owing to laboratory limitations, the cooling device was primitive and inconvenient; by dint of careful control, however, it was made to give reliable service and the variations of temperature while it was used were the same as at other times. (Exact figures: the mean change per hour at 16° and 20°—the two levels where the cooling device was used—was .0059°, the median was .0040°; as compared with .0056° and

[7] Cf. Brown (*7*, 8) and Urban (*26*, 1 ff. and 173).

.0033° for the whole of group I, excluding 16° and 20°.) It consisted of a large reservoir, filled with ice and water, whence cold water was siphoned into each jar through rubber tubes, the flow being properly regulated with screw-clamps. The surplus water was siphoned into a receptacle below at such a rate as to keep the water-level about the same in all jars. For work of this kind one should have a refrigerating device much more delicately regulable than flowing water; but the figures above cited may lie in evidence that the reliability of the data was in no wise affected by the limitations of the apparatus.

For securing accurate measures, a Beckmann thermometer was used throughout. In the method of limits, where the temperature of the variable rises or falls continuously within a given series, it was indispensable for getting the stimulus-value quickly and accurately at the moment of O's judgment. It was of the highest value in the constant methods as well, for setting and checking the constants. In group II another thermometer was placed above the table from which the air temperature was daily recorded.

CHAPTER IV.

Description of Procedure

The general procedure, whose definitive form was reached in the constant methods, will here be exhibited in detail; any departure therefrom, in the use of other methods, will be noted in connection with the appropriate data.

1. *Observations by the Writer*

In using this method, by which nearly all the data were secured upon which the conclusions of this study rest, the writer[1] did practically all the observing himself, as will appear from the table of results. The data by the method of limits are largely from other observers, but include some from E-O. For the director of an experiment to do most of the observing, especially when the procedure must be "without knowledge", is not an ideal method. It was adopted in this study, not from principle, but of necessity. Psychophysical investigations being somewhat tedious, and much more time-consuming than ordinary psychological experimentation; being also out of line with the interests of many present-day students in our field, it was found impracticable to get O's who were willing to give the time and devotion that such quantitative work inexorably demands. E-O therefore decided to make his own observations as full and extensive as possible, in lieu of fragmentary results from a number of other O's. His data, accumulated in a period of over four months, occupied, for the whole of that time, from 17 to 24 hours a week, that is, three to four hours a day, of actual observation.

The merits and demerits of this policy claim a few words. In favor of E's observing are several undoubted benefits: 1) Extensive series of data can thus be secured from the same subject—a cardinal necessity in this type of problem. The irregular and limited accumulations often resulting when the convenience of other O's must constantly be consulted are thereby avoided. 2) E's interest in the process of taking data

[1] To be designated hereafter with the symbol E-O, in virtue of being director of the experiment (E) as well as observer (O).

is supported by incentives which no other O can be expected to have in the same degree; he is therefore likely to be more attentive, accurate and consistent in his judgments; his wider knowledge of the topic enables him better to note and interpret the ancillary factors to the problem under examination. 3) E thus receives, finally, a concrete and detailed knowledge of the whole process upon which his data depend, such as he is not likely to get if he confines himself to recording the observations and comments of others. The policy, on the other hand, is not free of disadvantage: 1) The data being largely from one O, critical collation with those drawn from other sources under like conditions is impossible. Generalization becomes more precarious, and the insight to be gained from comments by other trained observers is lacking. This shortcoming does not seem to be crucial here, since our concern is rather in the relative performance of the same O under different conditions than of different O's under the same conditions. E-O's absolute thermal sensitivity may well differ from that of other individuals; but our first interest, in this study, lies elsewhere. 2) The need of keeping E-O in ignorance of the actual stimulus-differences made it necessary to commit the setting of the temperatures to assistants. These were chosen from students (mostly graduate) in Psychology and modestly remunerated. Their service was, on the whole, quite commensurate with their tasks;[2] so that the work was probably done about as well as though carried through under E's direct supervision. 3) A third consequence of the method was that the results could not be tabulated and treated until experimentation was complete. This much delayed the final phases of the work and compelled E to proceed partially without the guidance which early data

[2] Those engaged in the vital work of setting and checking the temperatures were chosen with especial care: Miss Ruth Miller, Laboratory assistant in Psychology; Miss Edna Willis, graduate student; Dr. J. C. Reamer, then graduate student; Miss Deyo, school teacher; all the above were mature and capable persons. Further, R. B. Gordon and N. C. Schwenk, undergraduates, whose care and fidelity over a long period were highly praiseworthy; and several other students, whose service was brief but on the whole adequate. The same individual almost never read the temperatures twice on the same day; they were commonly set by one assistant and checked by another; so that our discussion above, showing the small mean change between successive readings of the temperatures, may also go to prove that the assistants did their work very well indeed. To all those named we wish here again to acknowledge our indebtedness; in especial degree to Miss Miller, whose unfailing interest and help were invaluable.

can yield for the future course of an investigation.[3] In spite of these facts, this policy was consistently held, to prevent any disturbance of future results by the knowledge of previous ones. Specially important was this policy because of constant errors due to method. Definite knowledge about the presence and extent of such an error leads almost certainly to one of two results: either to its gradual elimination, as O begins to make allowance for it in his judgments, or to its accentuation through O's effort to avoid the other alternative. It is also conceivable, in a study which seeks to determine whether a law, already formulated and threshed over in other fields, holds also for this sense, that knowledge of preceding results might subtly warp O's future observations in favor of the theoretical demands of the law. Owing to the great complexity of the situation, this result, to be sure, is not very likely; but the possibility can hardly be waved aside as purely fanciful.

The writer submits therefore that none of the *a priori* drawbacks just cited were of consequence here: the first, for the reason given above; the second, owing to the competence and fidelity of the assistants; the third, because of the writer's extensive preliminary experience with the whole range of problem and procedure involved.

The method (whether simultaneous or successive presentation of the stimuli, as well as the adaptation temperature) and, more especially, the constants to be used each day, were fixed in advance by E-O in accord with a systematic schedule; the assistant then arranged these temperatures in an order known only to himself among the several jars; when comparing two stimuli, therefore, E-O had no clue whatever to their actual disparity. E-O knew the original temperatures that had been set for a series and about what differences were present; but he took every precaution *not* to know how these intervals were distributed, so that his information was strictly limited to these points: (1) roughly, how many intervals of each size—0.05°, 0.10°, and so on—were represented in the series; more

[3] This sentence must not, however, be interpreted to mean that the writer was entering upon his experimental work with a kind of self-imposed blindness. A large body of material had already been collected from four different O's by the method of limits, during which period the writer had personally attended to every detail from apparatus to protocols; and had also spent about three to four weeks in personal observing by the same method. He thus had abundant opportunity to acquaint himself with almost every problem of apparatus or procedure, before beginning the constant method at all.

specifically, (2) whether any two jars had been set equal, and (3) that no two jars differed, in the original setting, by more than a certain amount, say, 0.25°.

We thus start the question: Given this order of knowledge by E-O, was the procedure still "unwissentlich" in every essential regard? Did E-O's knowledge that more or fewer .00° intervals were scheduled for a given series affect in any way the likelihood of a "doubtful" or "equal" judgment in those observations ("equal" or "doubtful" being the objectively "correct" judgment[4] for a difference of .00°)? The observer might incline to give more "doubtful" judgments if he knew that several pairs of jars were equal than if only one pair were the same; or he might tend to compensate and thereby displace his judgments in the opposite way.

On this point we can offer almost no figures, because two jars, at least, were regularly made equal; so that we have but three cases which differ only in the percentage of .00°-intervals. For these three, an increase in the proportion of .00°-differences is twice attended by a decrease in the number of "doubtfuls" and once by an increase. While the figures are too meager to prove much, they at least suggest no correlation between these two factors.

Other facts bearing on the general question are more conclusive. 1) As already elaborated, the temperatures often rose or fell during the day by one or more hundredths; the intervals between them were thus correspondingly affected. Of two jars differing by .05°, if one rises .02° and the other drops .02°, the interval becomes .01° or .09°. The original distribution therefore seldom remained wholly unchanged throughout a working-period; E-O, being fully aware of these facts, was thereby preserved from allowing his knowledge of the original setting to modify his judgments. 2) The judgment 'doubtful' may seem most likely to appear when the difference to be compared is .00°; but this is by no means always the case. Because of constant errors due to method, the modal "doubtful" often fell above or below .00°. Table II gives the distribution. We find that only 15½ in 50 (31%) fall at .00°; that is, in 69% of all cases, "doubtful" was less likely to appear between equal temperatures than between those separated by .05° or more. Even if E-O knew therefore that one series had twice as

[4] It may be noted that, by the close of the practise series, "equal" had been supplanted by "doubtful."

many .00°-intervals as another, he was equally aware that this had no fixed causal relation to the "doubtful" responses at all; it might increase or decrease their number according to the extent and direction of the constant error. The amount of this error being wholly unknown, even though its direction might be inferred, E-O was well assured, in our opinion, against any tendency, conscious or unconscious, to bias his judging from what he knew about the number of intervals. 3) In Table III is summarized the relative number of the sev-

TABLE II

+.25°	+.20°	+.15°	+.10°	+.05°	.00°	—.05°	—.10°	—.15°	—.20°
1	1	2	5	7½	15½	11	4½	1	1½

Distribution of the crude modes of the "doubtful" judgments for all temperatures in our principal method (A; see p. 57); 50 cases in all. When two intervals had the modal frequency, each counts one-half. The 1 under + .25° means that, in one case, the largest percentage of "doubtful" responses appeared when the comparison jar was .25° warmer than the standard.

eral stimulus-differences, as drawn from the original schedule prepared by E-O. We find more small intervals at the levels where discrimination became more accurate; but apart from this will be seen that the percentages vary little indeed from one level to another. This uniformity in the scheduled distributions served to reduce still further any possible effect that knowledge of them might have.

TABLE III

	16°	20°	24°	28°	32°	36°	40°	44°
.00°	.05	.08	.11	.13	.20	.13	.07	.07
.05°	.40	.41	.40	.47	.53	.53	.40	.40
.10°	.33	.28	.33	.30	.27	.27	.33	.33
.15°	.15	.16	.13	.10	—	.07	.13	.13
.20°	.07	.07	.02	—	—	—	.07	.07
.25°	—	.01	—	—	—	—	—	—
Number of intervals	40	155	45	60	30	30	30	30

Distribution of stimulus-differences scheduled in method A (practise series omitted). How to read the table: of 40 temperature-differences scheduled for use at 16°, 2 (= .05) were .00°, 16 (= .40) were ± .05°, and so on.

(4) The above facts may serve to disprove any influence by E-O's knowledge upon his responses, even had this partial information been always before his mind; but of course noth-

ing of the kind was true. The differences were taken, each for itself, as revealed by the sense-impression. By the close of the practise period, the judging process had become well standardized and proceeded in the objective and uniform way which work of the kind, when long and carefully controlled, inevitably assumes; hence was all the less open to distortion by extraneous factors of any sort.

The facts above cited, in our opinion, amply endorse our procedure as being "without knowledge" in every essential, despite the minor departures from ordinary practise necessitated by E-O's position as both director of the work and observer.

2. The Stimuli

The following temperatures were worked in the order shown: Group I (February 10-June 2) 32, 28, 24, 20, 16, 20, 24, 28. Group II (July 2-August 11) 48, 44, 40, 36, 32, 28.

Of the seven jars on the table, six were regularly used; each one served in turn as the standard, the other five being then comparison jars; 15 pairs, or temperature intervals, were thus formed: 1-2, 1-3, 1-4, 1-5, 1-6, 2-3, 2-4, and so on. The seventh jar was kept regularly at 32° (approximate skin temperature of the fingers[5]) and served as adaptation jar in the method of Equivalents and as a reserve for use in case one of the others was temporarily out of order. As previously stated, the constants to be used each day were fixed in advance by E-O in accord with a systematic schedule; for example, such a series as the following would be reserved for two successive days:

19.95°
19.95
20.00
20.05
20.10
20.15

[5] The following measures of skin-temperature on the hand may be cited from Nagel (*19*, I, 559 f.) as typical, the concomitant air-temperature being added in parentheses:
Kunkel: back of hand, 32.5° to 33.2°; palm, slightly higher (20°);
Stewart: palm, 31.0° (ca 18°);
Rubner: unclothed surfaces, in the mean, 31.7° (25.6°).
Benedict and Slack in 1911 (*4*, 4) resume the facts on surface-temperature in these words: "as a result of numerous observations made by different methods, 32° C has been commonly accepted as a standard, and is probably not far from the true value".

on the first day, the assistant would allocate these values in an order known only to himself among the six jars; the next day the values were reversely arranged, so that jar 6 had the temperature of jar 1 the day before, jar 5 of jar 2, and so on; exactly the same intervals were then present but in opposed space-order. The above sample set yields 15 intervals, distributed as follows:

0.00°	0.05°	0.10°	0.15°	0.20°
1	5	4	3	2

It is obvious that no possible disposition would give an equal number of intervals for each size: e.g., no arrangement could give three values in each interval above. This being true, the ordinary method by which every interval is compared the same number of times was not feasible. Our method on the contrary provides *larger* stimulus-differences and relatively *more small* S-D as well, than does the conventional procedure. We thus open the question: How does it compare with the latter?

(1) The range of stimulus-differences, each temperature in turn being standard, is about twice as wide as in the usual method; some extreme values of p (near .00 and 1.00) are thereby insured in every case; these, being notably more reliable, have correspondingly greater effect upon precision and limen than does a p near .50.[6] We thus save time and energy by including some large S-D along with smaller D whose percentages (of warmer and cooler) depart little from .50. In this connection a table was compiled, giving the range of p_w and p_c for every set of observations (80 in number) from which a limen was calculated in method A. In 48 of these (= 60%), p ranges from .00 to 1.00; of the remaining 32 (summarized in Table IV), 26 range from .70 to .99 and 6 fall below .70. To clarify the meaning of these figures, let us consider an illustration: in the set of temperatures employed for determining one of the warm limens at 24°, two were so much lower than the standard that the response "warmer"

[6] Both Urban (*25*, xvi, 182) and Boring (*5*, 285) contend that a p near .50 affects the position of the limen more than does any other; a plausible error, since the Urban weight, P, is maximal at p = .50 and minimal at p = .00 and 1.00. But the facts are precisely reverse: when p = .01 or .99 it affects the course of the fitted curve more by 25 times than does p at .50. For discussion and proof, see article IV of the writer's "Studies in psychometric theory" (*7b*).

never appeared ($p_w = .00$), while one was so much higher that "warmer" always appeared ($p_w = 1.00$); in the opposed space-order, the lowest p_w was again .00 but the highest was .92. We find, now, in 60% of the cases from which limens were derived, that there were a few differences so obvious that a "warmer" (or "colder") response never appeared with some of them and always appeared with the others; while in 74 of the cases (= 93%), the intervals were wide enough to give p a range of .70 or more. These facts may serve to show that while, in general, not a great many large S-D were used, there were enough to give *some* high and low values of p (near .00 and 1.00) in every set.

(2) In addition to a few large S-D, our method provides many smaller D (.00°, .05°, .10°) as well. In view of (1) above we may be tempted to ask: why not use nothing but large S-D, omitting the smaller ones wholly? Our p would then all have high reliability and weight. To such a plan three facts are decisively opposed: (a) When we try to determine any theoretical function, sound policy invariably demands as many empirical measures as possible; the more observed values the better. In lieu of the old method of two or three stimuli, we now use complete series of S-D (Vollreihen) with the express design of gaining an empirical picture of differential perception through the range from 0 to 100 per cent. The small S-D with p near .50 then are indispensable and can be made as reliable as the extreme percentages by simply taking more cases; which is just what our procedure affords. (b) O's attitude and performance depend on the number and relation of the stimuli; his response to any one is conditioned by others gone before; were there no small S-D,

TABLE IV

SHOWING THE RANGE OF P_W AND P_C (OMITTING 1.00)

		.90 to .99	.80 to .89	.70 to .79	.60 to .69	.50 to .59	.40 to .49	.30 to .39	Total
p_w	I	2	1	1		1			5
	II	2	1	4	1				8
p_c	I	2	3	3			1		9
	II	3	2	2	1	1		1	10
Total		9	7	10	2	2	1	1	32

the larger D would be otherwise perceived than they now are. (c) Small D finally provide an incentive for the alert observer and give the comparative process a kind of "intellectual" interest; a few easy judgments are useful in keeping O "steady" and confident (*2*, 282), but when at all numerous they tend to make the work monotonous and perfunctory. In our method accordingly we are assured the benefits of a few easy comparisons and have enough small D as well to make the medial percentage-frequencies reliable.

(3) How finally does our procedure meet the following consideration (from Boring; *2*, 282): if a narrow range of stimuli be used, a small departure of the limen from its expected value will give an asymmetrical distribution of the stimuli about the threshold and thus tend to arouse habituation or expectation in O. In other words, a predominance of one judgment (warmer) will tend to make it habitual or expected, and thus artificially raise the value of p_w in that set of observations; hence the stimuli should be so distributed that warmer and cooler occur about equally often. There being two time- and two space- orders, we have four possible combinations of them; a separate limen was derived for each of the four at every temperature covered in our study. Now in our data we find, if the four time-space combinations are viewed separately, the proportions of W and C sometimes differing widely. Consider, as an actual case, the results for 16°:

Time-order	Space-order	W	C
1	1	.19	.54
1	2	.38	.36
2	1	.46	.29
2	2	.48	.26
Arithmetic mean		.38	.36

Of all the observations made at this temperature, time- and space-order 1, we see then that 19% were W and 54% C (the remainder, 27%, being "doubtful"). The point of "subjective equality" (Urban: *22*, 16, 201), labelled Xi, here equals +.112°; this means that the responses W and C are equally probable, not when standard and comparison-stimuli[7] are the

[7] For brevity, the standard will hereafter be denoted by S; the comparison-temperature (stimulus judicandus) by J.

same, but when the latter is .112° higher than the former; the excess of stimuli below this point of course induces a preponderance of C judgments. In the opposed time-order we find W = .46, C = .29; Xi being here located at —.097°, W responses predominate. The disparity in the values of Xi is, of course, largely due to exposure of the judging-surface to the air in passing from S to J; this constant tendency, in theory, is equal in amount and opposed in sign in the two orders. Now even if we assume that the overplus of C's in the first order induced a bias or expectation in favor of C, the opposed bias in the second order would tend to neutralize it, when the two are combined in an average. Under the necessary conditions of our study, the four time-space combinations all had to be used with the same setting of stimulus-differences; whence it follows that the point Xi, as in the examples above, was bound to shift either up or down, according to the direction of the constant error in each combination. Now, inasmuch as the limen at any level is the arithmetic mean of the limens for the four time-space orders, it follows that we must also use the mean of the responses W and C to find how the limen was affected by their relative frequency; see Table V. From the means we find, omitting practise series, that W

TABLE V

Showing the Mean Percentage-Frequency of the Judgments W and C at Each Temperature-Level (Method A)

	28°	24°	20°	16°	20°	24°	44°	40°	36°	32°	28°
W	.42	.39	.378	.38	.39	.42	.32	.44	.40	.41	.38
C	.35	.42	.383	.36	.36	.33	.48	.34	.32	.31	.35

Each value is the mean of four frequencies, one for every time-space combination. The first two columns (28° and 24°) contain practise series.

exceeds C in 7 cases, and falls below it in 2. The difference never exceeds .10, except at 44° which is anomalous both in extent and direction of difference. It may be of interest to note that, in Urban's study (*25*, XV, 287) of lifted weights, conducted with every precaution under conditions which may be considered ideal, appear the following percentages of "heavier" and "lighter" judgments for his seven O's at the one level which was used (100 gm):

	I	II	III	IV	V	VI	VII
H	.37	.38	.38	.42	.46	.33	.33
L	.41	.47	.57	.48	.44	.48	.49

The mean of the differences, sign not regarded, is .101; the same mean for our data (Table V), practise series omitted, is .068. Our results seem therefore to compare favorably with Urban's in this respect.

The reader should be apprised that, on p. 62 below, we come back to the question with new light from the value Xi. By its use we find that, even when the frequencies of W and C differ by as little as in Table V, O's judgments are apparently skewed to some degree in favor of the larger figure. Fortunately Xi corrects for the tendency as well as revealing it, by showing us where the subjective "boundary-line" between warm and cold lies. The danger of expectation and habituation in the process of judging is thus confirmed; but the remedy customarily proposed (cf. Boring: *5*, 282, as cited above), of keeping the stimuli symmetrical about the limen, is quite inadequate. It is no easy matter, as the data from Urban indicate, even where conditions are much simpler than in our study, so to distribute the stimuli as to get the same percentage of opposing responses; but Xi offers an extremely useful way of eliminating what error creeps in by this source.

Our use of a variable standard is therefore, in our opinion, not merely defensible but highly advantageous as well.

The temperatures having been set and checked, the process of observing was ready to begin. Nearly all the observations were taken between the hours of noon and five p.m., and were recorded by assistants. A cardboard stencil had been prepared with 120 holes, arranged in six sections of 20 each; through these openings the judgments were recorded on the page beneath. The sections were numbered from I to VI, to tally with the six jars. The five holes of each row in section I were marked for the five possible combinations of jar I with the others; the rows were alike except for the *order* in which the comparisons were made; in each row the judgments were taken and recorded in a different, but fixed, order, as follows:

First:	1-2,	1-3,	1-4,	1-5,	1-6,
Second:	1-4,	1-2,	1-6,	1-3,	1-5,
Third:	1-3,	1-2,	1-5,	1-4,	1-6,
Fourth:	1-5,	1-2,	1-4,	1-3,	1-6.

By this device any two jars were compared four times in a full series, but each time preceded by a different pair; for example, the comparison of 1-6 was preceded, in the first row,

by 1-5, in the second by 1-2, in the third by 1-4, in the fourth by 1-3. The same plan was used in the other sections. This arrangement was designed to make innocuous the influence that any judgment is likely to have on the one immediately following. It is well-known, for example, that if the preceding variable was distinctly cooler than the standard, the next one, slightly cooler, may be judged doubtful or even warmer, simply because the difference is less conspicuous than the preceding; whereas apart from this influence the judgment might have been, correctly, cooler. The above arrangement served to obviate any uniform effect of this sort.

The recorder, who sat at a table nearby, read the combinations in their stated order and as each one was called, O gave his judgment. At the end of a series (covering about 45 minutes), 120 judgments had thus been taken.

As above stated, every combination, or pair, of jars was judged four times in each order in the course of a series; for example, jars 1-2 were compared four times in that order and also four times in reverse order, 2-1; eight comparisons for each of the fifteen combinations thus make up the total of 120 judgments in a series.[8] In addition, three or four series were regularly completed in an afternoon. It will at once be seen that this procedure departs from customary usage, in which the stimuli are presented to O in such wise that he cannot possibly know, when comparing any two, what judgment he gave to the same pair before. For example, in Urban's study, the weights were arranged in an order unknown to O on a rotating table which was screened from his view; it was thus easy to keep him from knowing with which pair of weights he was dealing or what judgment he had given when estimating them before. Any such control was out of the question under the necessary conditions of our experiment. The bulky and elaborate mechanism precluded any manipulation of the jars and the temperature-intervals, of course, remained constant; so that, in most cases, E-O could not avoid remembering, as he repeated a given comparison four times successively in a series, what estimate he had previously given to that same pair. The question therefore arises and demands careful attention: Did the fact that E-O compared what he knew to be

[8] In comparing jars 1-2 (1 being the standard), E-O gave his judgment in terms of 2; in the opposed order 2-1 (2 now being standard), the judgment applies to 1. In the former case, W means that 2 is warmer than 1, in the latter that 1 is warmer than 2.

about the same interval as often as 16 times in an afternoon, in each time-order, with no change in the original setting, give evidence of modifying the later judgments?[9]

Now there are two conceivable ways in which our procedure may have biased the judging process and thereby warped the results; whence our question thus divides: 1) Did E-O modify his later judgments, either positively or negatively, in consequence of having compared the same jars before? 2) Were the inverse trials affected by the antecedent primaries (stimulus 5 might be called warmer than 3, say, because 3 was previously judged cooler than 5)? To answer this two-fold ques-

TABLE VI

		I	*II*	*III*	*IV*
Rights	*Primary*	63 65 56 64	57 55 53 54	61 67 65 65	58 65 61 67
	Inverse	59 63 63 57	62 64 64 63	55 53 54 60	60 64 60 59
Wrongs	*Primary*	11 11 13 10	15 14 17 18	7 11 10 8	10 8 12 13
	Inverse	13 14 12 9	13 9 14 10	11 16 8 15	14 12 11 9

Table VI. Showing the mean percentage-frequency of Right and Wrong judgments for the four successive trials in each series at all the temperatures, from 16° to 40°, of which limens were secured by our principal method (A; see p. 122). The data by this method are the most extensive in our study, and what is true of them may be received as typical of "constant stimulus-difference" findings. The figures at 44°, it may be noted, being limited and unreliable as later shown in detail (p. 59), were not included.

By a "right" judgment we mean one that agrees with the objective difference; noting, however, that this is reckoned not from .00°, but from the point Xi in a given series. At 40°, time-order S-J, space-order Right, for example, Xi equals —.075°; that is, the responses W and C are equally probable, not when S and J are the same, but when J is .075° lower than S. Hence, for all stimulus-intervals above —.075° (such as —.05° or +.05°) Warmer is called right and for those below Xi (like —.10°), Cooler. This was done to free the percentages of constant errors and thus make them really comparable.

In a working-period four series were usually completed, here designated I, II, III, IV. Any two jars (as 3 and 5) were first compared in one order (3-5), hereafter called the "primary" one, and soon thereafter in the opposed order (5-3), called "inverse". At the beginning of a day's work, the 15 combinations for that period were compared for the first time. Of these initial judgments in I each day, for all the temperatures, 63 per cent were right and 11 per cent wrong; the second time through, 65 per cent were right, and so on. Directly after the primaries came the corresponding inverse, with a mean of 59 per cent right, and so on.

These figures are merely for illustrative purposes; hence the means are rounded off to the nearest integer and their deviations are not recorded.

[9] For example, in the case of a judgment originally "doubtful", repetition may refine it to the correct one, W or C, and thus artificially decrease the limen below the value it would have were the previous trials unknown to E-O.

tion, upon which depend the worth and reliability of our data, a careful survey of all the material upon which our calculations are based was made.

The results of this survey were compiled in extenso and are here epitomized in Table VI and in the text. The mean percentage-frequency of both "right" and "wrong" judgments in the successive trials of each series, for all temperatures, are shown in the table. The purpose of these figures may thus be stated: if the observer has anything like a well-defined tendency to meliorate his later or inverse judgments in consequence of have compared the same stimuli before, it is bound to emerge either as an increase of right or as a decrease in wrong responses; if he is "set" to compensate, the opposed result will follow. If the proper collation of our data, therefore, reveals significant or uniform changes from earlier to later or from primary to inverse trials, we shall needs infer that a bias of this kind has been operative; if, on the contrary, we can wring from these figures no such evidence, we are surely entitled to deny any appreciable influence upon our data from this source.

(1) *Are the inverse judgments affected by the preceding primaries?*

a) Among the differences between primary and inverse percentages, how many do we find of significant size in each category (plus and minus)?

In a given series all the primaries were completed before the inverse trials were begun, whence any of the latter were open to disturbance from all the former. Comparing each to each, therefore, we have 16 differences (positive, negative or zero) in a set (cf. Table VI), or 64 in all at any given temperature. In some cases the inverse means, based on the seven temperatures from 16° to 40°, exceed the primary, in others fall below; from their deviations we may determine, in the usual way, if their disparity is "significant."[10] Comparing the means we find such changes as follows:

[10] The term "significant" is here employed, of necessity, in a kind of Pickwickian sense. A mean and its deviation, when derived from only seven values, can have but limited reliability, even though the underlying percentages are each based on some thirty cases and thus have a fair degree of stability. But the important matter for us is not whether any particular disparity is significant, but only the relative number, in a sizable group, of plus and minus changes that meet our criterion. The 64 cases in our table would seem enough to reveal any material bias, up or down, that may exist.

	Number of cases	Increases		Decreases	
		number	per cent	number	per cent
Rights	64	5	7.8	5	7.8
Wrongs	64	3	4.7	1	1.6

Ten Rights and four Wrongs meet our criterion, the per cent ranging from 1.6 to 7.8. In brief, the table shows: 1) A small percentage of material changes; 2) What is of more import, in the Rights positive and negative are just the same; these equal and opposed results can hardly be ascribed to a common or uniform cause; and since primary and inverse determinations combine equally in fixing the limen, these changes are mutually nullified in the end. 3) Final interpretation of the apparent increase of Wrongs will be deferred until after 2, b.

b) Are the changes from primary to inverse uniform in direction, without regard for significance of size?

Upon comparing, as above, each primary with all the inverse in a given series, we get 64 changes in a full set of four; but we now use the seven underlying temperatures instead of only the means; and thus have 7 times 64 or 448 changes, with no regard for magnitude. If the inverse frequencies run above or below the primaries, it will appear in these figures (for simplicity, cases of no change are equally apportioned to increase and decrease):

	Number of cases	Mean of		Deviation from equality (.500)
		increases	decreases	
Rights	448	.506	.494	.006
S.d.		.024	.024	.024
Wrongs	448	.498	.502	.002
S.d.		.024	.024	.024

Among the Rights were 210 increases and 205 decreases (33 being equal), with the Wrongs 194 and 196 (58 equal); whence the means above given. Were there no trend up or down, all the means would be equal (.500); it follows that their deviation from equality measures the degree of bias.

In view of the limited reliability of any form of deviation when based on seven values, the m. d., being easier to compute, was used in lieu of the s. d. Two one-half times the m. d. (= about twice the s. d.) was taken as the criterion of "significance." The deviations are not shown in Table VI, which is included merely to help elucidate our procedure.

This figure in both cases is a mere fraction of its own s.d.; but we need no *apparatus statisticus* here to show how meager and casual these deviations are. If the inverse determinations were ever so gently skewed up or down in consequence of antecedent primaries, the fact should emerge from this table; as it is, no real bias of either sign can be detected.

(2) *Are later judgments affected by earlier comparisons of the same stimulus-differences?*

a) Among the differences between early and late percentages, how many do we find of significant size in each category (plus and minus)?

Upon comparing each mean in Table VI with all that follow in the same series, we have six changes, or 24 in the four; and six additional values from the four series-means. By means of our criterion (2½ m.d.) we again locate the "significant" ones.

	Number of cases	*Increases*		*Decreases*	
		number	*per cent*	*number*	*per cent*
	1. From earlier to later trials in same series				
Rights	48	0	0	0	0
Wrongs	48	0	0	2	4.2
	2. From earlier to later series				
Rights	12	2	16.7	2	16.7
Wrongs	12	1	8.3	2	16.7

In not a single case, among 48 Rights, do we find a reliable difference within a series. With the means of whole series, changes are indeed found, but again (as in 1, a) positive and negative are equal; whence their only effect is mutually to nullify each other. Out of 48 Wrongs, two fall off with no correlative increase; in the general means two drops are opposed by one rise. Interpretation of this decrease in Wrongs is reserved until after 2, b.

	Number of cases	*Increases*	*Decreases*	*Deviations from equality (.500)*
Rights				
Total amount of change	1680	837	843	
Mean amount of change	1680	.498	.502	.002
S.d. of mean		.012	.012	.012
Wrongs				
Total amount of change	1680	744	936	
Mean amount of change	1680	.443	.557	.057
S.d. of mean		.012	.012	.012

b) Are the changes from early to late uniform in direction, without regard for significance of size?

As in 1, b, we now consider every figure of the four series at the seven temperatures used. Comparing each percentage with all that follow in the 16 trials of the 4 series, we have 120 differences or 1680 for primary and inverse at the 7 stimulus-levels.

In Rights, these figures speak for themselves: out of 1680 cases the positive and negative changes differ by just 6—less than four-tenths of one per cent. The Wrongs, in both primary and inverse, have an excess of negative changes, which may be taken as reliable;[11] the tendency of 2, a reappears here in more positive form.

Assuming a real tendency to give fewer Wrongs (that is, more doubtfuls) in later trials, let us see what follows. If the reader will consult the diagram on p. 54, the following analysis may be clearer. Assume the curve there shown to be the psychometric function for Warmer. The position of the median marks the limen, the crude limen being reckoned from the point .00°, the true limen, free of constant error, from the point Xi. In our tabulation of Rights and Wrongs, all warmer judgments for temperatures higher than Xi are "right", for those to the left of Xi are "wrong"; hence the only Wrongs which in any wise affect the course of the Warmer-curve are those in the tail to the left of Xi. If we know where Xi lies, we can thus determine just how much the curve will be disturbed by a change in the number of Wrongs. Xi was located for all the limens here involved, with this result: in the Warmer curves Xi is, in the mean, 3.0 p.e. below the median and in

[11] The customary criteria of significance are here of little value. As in 1, b, the distribution is far from Gaussian, being a tall, narrow trapezoid. With the Right Increases, for example, the base runs from 0 to 1, the zero ordinate being 887, the unit ordinate 793. In a normal curve, over 99.99% of the area is included within ± 4 s.d.; here, about 10%. Otherwise stated, in a normal curve all the values entering into the mean lie within 4 s.d. of it; here every value is either 0 or 1, that is, 40 s.d. from the mean, whence a trivial disparity of the two ordinates shifts the mean violently. The only criterion known to the writer that seems to be suitable here is the theorem of Tchebycheff (cf. Fisher, *12*, 115 and Czuber, *8*, 232 ff.); by which the probability that any of our percentages (such as .472) deviates, positively or negatively, from the expected *(a priori)* value (in our case, the mean from a large number of series of 1680 each) by more than t times its s.d. is less than $1 - (1/t^2)$. In the table above (Wrongs), $t = (.057/.012)$, whence $1 - (1/t^2)$ equals .956; the Wrongs therefore deviate from equality (.500) by an amount whose validity exceeds 1.71 s.d. in a normal surface. This deviation is little, if at all, above the limit of significance; to be conservative, however, we accept it as genuine.

the Cooler 3.0 p.e. above. From a Sheppard's table we get the ordinates to the left of Xi as well as to the right, and from their indices of reliability, 1/pq, find how much they count in defining the course of the whole curve. The Wrongs prove to have a mean weight of roughly .22, the Rights of .78; that is to say, if *all* the Wrongs were changed to doubtful (the Rights being unaffected), the total curve would be modified in the sum of 22%. But only 5.7% of the Wrongs are involved; were that proportion of the changes from Wrong to doubtful reversed, an exact balance would be restored. .057 of .22 is 1¼ per cent; and even this overstates[12] the mean net influence upon our psychometric curves from the excess of minus changes in the Wrongs.

To sum up: all the data secured by our principal method with constant stimulus-differences are collated; they are freed of constant errors and then scrutinized for changes of significant size or uniform direction from primary to inverse and early to later trials; in the Rights, nothing beyond the most petty and casual bias can be discerned at any point; in later trials the Wrongs apparently give way in favor of Doubtfuls; this tendency, though not beyond question, is assumed to be genuine; even so its mean effect upon the psychometric function proves to be altogether negligible.

From this evidence, in our opinion, we may with confidence assert that our mode of procedure introduced no errors of uniform trend or perceptible size.

Several considerations may here be cited in explaining this result. Let us note, first of all, that no mere repetition of the initial reports, however well they may have been done, would begin to achieve the consistency above revealed, for two reasons: 1) as previously shown, the actual stimuli often rose or fell by one or more hundredths in the course of an afternoon; whence the intervals between them were altered, and 2) the

[12] For the reason that the factor of change (1¼%) operates in two opposed ways at the same time. Suppose a S-D of —.10° is at first wrongly called warmer and later changed to doubtful (the situation here); this change will have two antithetical effects: it will raise the precision (h) of the Warmer function, thereby tending to lower its limen; and at the same time will force the median away from Xi, thereby raising the limen. If O e.g. gave no doubtfuls at all, the Warmer and Cooler curves would coincide at their 50%-points and both limens reduce to zero; while if he always reported doubtful, however large the S-D, the limen would become infinite. As the doubtfuls increase, therefore, L tends both to rise and to fall; in other words, the factor of change nullifies itself and becomes in practise nugatory. Cf. *7b*, article IX, for ampler discussion of this point.

space- and especially the time-orders were infected with constant errors. If J is pronounced warmer in order S-J, in the opposed order (J-S) S may also be rated warmer, depending upon extent and direction of the constant error, whose degree was wholly unknown to E-O while its sign could be only inferred. For both reasons, a fresh and careful scrutiny was always in order. In the second place, a careful and experienced observer is less open to bias in a situation of this kind than may be imagined. For one thing, preceding comparisons of the same stimuli were seldom all alike; even if they were, E-O by no means assumed a given jar to be warmer merely because he had so rated it a few times before. Preceding estimates, so far as they came to mind at all, served rather as a kind of hypothesis to be scrutinized and appraised; his attitude to them was essentially pragmatic, non-committal. By the close of the practise period, a competent observer takes each pair of stimuli for itself, in impartial and routine fashion; he thinks not of preceding estimates but in terms of the sense-impression; and even though, now and then, an earlier estimate come to mind, he can take it in a critical and objective way. That this statement of a practised observer's attitude is not fanciful, our analysis of the data may serve to establish.

3. Process of Comparison

The process of judging was carefully controlled. At the beginning of an hour-series, the four fingers of each hand were placed in the adaptation jar; the surface of the water being between second and third joints, the fingers were well immerged, without wetting the rest of the hand. Both hands were thus retained in the adaptation jar, with the water freely circulating between the fingers, for a few minutes (three to five) ; the exact time depended upon the temperature and is given in each case with the results.

The question will inevitably emerge, and may as well be broached at this point: Was a period of three to five minutes adequate for proper adaptation?

If by adaptation we mean complete thermal indifference, sheer absence of warmth or cold (even were such a condition ever attainable), this period is certainly not, in general, adequate. No pretense is made that, by the end of five minutes, a temperature of 16° was wholly free of cold. But we may

define the term less rigidly, as does Ebbinghaus, for example (*9a*, 565): "die Abstumpfung der Empfindungen bei kontinuierlicher Fortdauer der objektiven Reize." The practical question then emerges; what degree of "Abstumpfung" is needed that observing may properly begin? In favor of 3-to-5 minutes consider the following evidence: (1) our own preliminary trials. No exact figures were taken; but it was learned that as good an approach to indifference could apparently be attained within a few minutes as by the close of an hour. Adaptation seems to begin rapidly and then continue at an ever decreasing rate, like an asymptote approaching its limit. This may be especially true of a part like the fingers, whose bulk is relatively small in proportion to superficial area. (2) the results of Holm (*13*, 243 ff). Upon applying a Thunberg temperator on various parts of the abdomen, the following times were needed, at the temperatures shown, for attaining a degree of indifference which was satisfactory to him (each value is the mean, in seconds, of 5 trials):

5°	10°	15°	20°	25°	30°	40°	45°
210	165	112	72	47	31	126	152

He thinks it may possibly take longer in adapting to a stimulus of larger area (he used a surface of about 5 sq.cm.); but after allowing for any difference of method, we may surely conclude that our adaptation-period (180 to 300 seconds between the limits 16° to 44°) was more than adequate by his criterion. (3) the results of Voigt. (*27*, 355). He immersed the hands separately in jars, which always differed by either 5° or 10° (the left one being, say, 25°, the right one 30°), up to the wrist (der Knöchel); and, after two minutes, attained the same degree of adaptation, by his mode of measurement, as could be reached in 70 minutes. From this he infers that adaptation "schon nach ganz kurzer Adaptations-zeit ein gewisses Maximum erreicht hat."

To the writer the above facts justify using an adaptation-period no longer than ours, even though indifference could not be wholly achieved in that time. It is perhaps commonly assumed that complete indifference is a precondition for accurate work in thermal discrimination; if this be true, the writer considers good work in this field impossible. We cannot survey the whole problem of adaptation with all its complexities; but we do face here a matter of method on which a few words are in order.

Abbott (*1*, 14 f.) maintains that her four observers, trained in psychological observation, all agreed that adaptation could be achieved "so complete that there was no sensation of temperature present." Though admitting it may seem *more* complete when both hands are exposed to the same temperature than to different ones, she states that in 600 preliminary experiments complete adaptation was reported in every case. Holm, on the contrary, says of his own figures, "dass sie an einer gewissen Willkür leiden. Es ist oft sehr schwer zu entscheiden, ob eine Temperaturempfindung aufgehört hat oder nicht. Oft meint man in einem Augenblick, die Empfindung sei verschwunden, im nächsten Augenblick, sie sei vielleicht doch noch vorhanden." He therefore regards only clear and unambiguous impressions. Voigt also records, as his first noteworthy finding, that even after 45 to 70 minutes, "niemals verschwand die deutliche Empfindung eines Unterschiedes der Temperaturen," which differed by 5° or 10°. The limit of adaptation must accordingly lie "far below" 5°, which he finds "auffallend gering", much less than generally supposed.

In this apparent conflict of inference and observation, the writer aligns himself with Holm and Voigt. Four facts emerge from a study of the comments by observers Geissler and E-O:

1) Adaptation was not always "complete" even at the close of a series, 45 minutes after it began.[13]

2) Indifference, even if partially or wholly achieved, could not always be indefinitely retained; it was subject to marked, though usually unexplained, interruptions.

3) Throughout an entire series, the adaptation-temperature might have different quality for the two hands: it would seem cooler to one or be indifferent to one and not to the other.

4) Complete adaptation seems to occur in the vicinity of skin-temperature, but is rarely, if ever, attained at the extremes; real indifference seems never to appear at 16° or 44° as it does around 28° or 32°. A good approximation to indifference, however, can be achieved even at the extremes.

Consider the following illustrative comments by E-O:

28°. "Left hand again seems cooler in adaptation-jar than right hand. Adaptation not complete by end of hour—water still seems cool most of the time; at other times seems much warmer—coolness is irregular."

[13] The fingers were not continuously immerged during this time, being exposed to the air about 5 seconds in every 20, during the act of comparing different jars.

"Late in hour, as well as earlier, 'waves' of coolness pass from finger-tips up toward hand."

20°. "Water in adaptation-jar felt indifferent or somewhat cool to left, but very cool to right, hand."
"Sometimes a sudden change seems to occur; a jar in which the hands have been adapting for some time will suddenly appear noticeably cooler to one or both than before."

32°. "In the series today, left hand feels distinctly cooler in adaptation-jar than right. This coolness much more conspicuous at some times than others."

These were reported by Geissler:

40°. "At end of adaptation-period (5 min.), water still felt warm."
"Apparent fluctuation in adaptation-temperature; it becomes alternately warmer and cooler."

Irregularities of type 2) were by no means customary; they are noted at 20°, 28° and 32° for E-O and at 40° for Geissler; they appeared suddenly and, so far as determinable, with no change in objective conditions, such as water or air. They illustrate, however, that complete indifference, even if once achieved, cannot always be uniformly maintained. The facts of 1) and 3) above were much more typical and widespread. They were by no means confined to extreme stimuli, but appeared with equal prominence in the vicinity of skin-temperature (28° and 32°).

It would seem obvious, in light of these facts, that a systematic program of investigation could hardly be carried through, were one to insist on complete indifference in both hands as a precondition of every judgment. Four practical impediments to any such demand may be cited: 1) Adaptation would occupy an indefinite period not determinable in advance; with the prior certainty that it could not be reached at all in many cases. 2) We should have to break off any series in case temperature-quality suddenly reappeared in either hand and continue "adapting" until this departed, if at all. 3) Adaptation is somewhat disturbed in the very act of comparing and would need to be reinduced, each time, to the point of subjective indifference. 4) If we persist *quand même*, keeping the fingers continuously immerged more than an hour, the skin becomes wrinkled and coarse and its normal sensitivity numbed; so that observing would have to cease until the skin was restored.[14] Surely, work hedged within this complex of limitations would be intolerably hampered. Finally, our survey of the data, given above, revealed no appreciable effect, in direction or degree, upon the accuracy of discrimination from be-

[14] In our procedure the hands were out of water 15 minutes or more between successive series.

ginning to end of a series; so that, if the small irregularities of adaptation played any role, they left no material mark behind them or their influence was neutralized by other factors.

Observing began, therefore, after a uniform period; three minutes near skin-temperature, four to five at higher and lower levels. The recorder then read the first combination to be compared; observer's hands moved in time with a metronome beating seconds. At a given stroke, the hands were lifted from the adaptation-jar; then two seconds in the air; two more in the first stimulus-jar; again two seconds in air, and two finally in the second stimulus; being withdrawn at the ninth stroke and returned at once to adaptation. Judgment was given as the hands were leaving the second jar, and applied invariably to J.[15]

The above procedure was appropriately modified in the method of equivalents; but in every case the time of exposure to a stimulus was two seconds, at all temperatures, and exposure to the air, preceding either stimulus, likewise two seconds.[16] The need for such regulation is apparent. The temperature of the hands, while passing through the air, either rises or falls (usually the latter). It is necessary that the amount of this change be the same in each hand, and also that it remain constant from one trial to the next. This can be well achieved by a fixed time of exposure. This change in the judging surface was an unavoidable feature of the method and introduced a constant error into our data; in the presentation of results will also be given the means used to eliminate this error. In this particular a device like Thunberg's Temperator (*23*, 385) may be less open to error, but is obviously unfeasible in other ways.

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[15] In comparing two stimuli, the response naturally applies to the second, as being warmer or cooler than the first; in time-order J-S, therefore, judgment appeared "mentally" in terms of S (say, cooler) but was reversed for record, thus applying to J (warmer). No perceptible difference in the movement of attention could be observed in the two opposed time-orders; the reversal in J-S was solely for uniformity and simplicity in the records. This was easily done, with a little practise, having, so far as could be determined, no distracting effect.

[16] An interval of two seconds was chosen because: 1) from experiments on attention, 1½ to 2 seconds is commonly accepted as the optimal time for reacting to sense-impressions, that is, for adjusting the attention to a given stimulus; it seemed therefore the best interval for exposing the receptors to the two comparison-stimuli; 2) it was long enough to enable E-O to pass from one jar to another without empressement and, at the same time, without keeping the hands in the air longer than necessary.

After the initial adaptation (say, to jar 1) the five combinations of that constant with the others (1-2, 1-3, 1-4, 1-5, 1-6) were each compared four times; number 1 being both standard and adaptation-jar throughout. The hands were then transferred to the second jar and, after one minute for adaptation, those five pairs were likewise compared four times; and so on, through the whole set of six. It will thus be seen that the adaptation-jar was also the standard, by this method; and that the adaptation-temperature varied, from one jar to the next, by the amount of their difference.

We thus start the question: How is the value of the limen affected by using each J in turn as a standard? We can easily prove that the error thereby introduced is wholly negligible. Consider a typical series of stimuli, as given in the left column of the subjoined table:

	Stimulus-differences				
	+.05°	+.10°	+.15°	+.20°	+.25°
20.15					
20.10	x				
20.05	x	x			
20.00	x	x	x		
19.95	x	x	x	x	
19.90	x	x	x	x	x

As the table shows, we have fifteen combinations or positive stimulus-differences, when all possible stimuli serve as S. With a given difference (say, +.05°), we should expect more Rights (Warmers) when S is 19.90° than when 20.10°; hence we need to look over the matter more closely. We see that 19.90° is the standard for every difference represented, from +.05° to +.25°; 18.95° for four differences (.05° to .20°); and so on. Now the Weber ratios run about .004 to .005 (p. 61); hence we may expect the limen at 20.10° to be about (.10° x .005) higher than at the true standard, 20.00°. Applying this procedure to all the S-temperatures, we get:

$$
\begin{array}{rrrr}
+.10^\circ & \times .005 & \times 1 = & .0005^\circ \\
+.05^\circ & \times .005 & \times 2 = & .0005^\circ \\
.00^\circ & \times .005 & \times 3 = & .0000^\circ \\
-.05^\circ & \times .005 & \times 4 = & -.0010^\circ \\
-.10^\circ & \times .005 & \times 5 = & -.0025^\circ \\
 & & \overline{15} & \overline{-.0025^\circ}
\end{array}
$$

Dividing by the weight 15, we get —.00017°; that is, the net error induced by our procedure in this case is about two ten-

thousandths degree. In view of these figures, words are hardly needed to show how inconsequential the error tends to be.[17]

Just 20 seconds elapsed from incipience of one comparison to that of the next; so that a full set of 120 trials occupied 40 minutes, apart from adaptation. One trial took 8 or 9 seconds; 11 to 12 thus remained for restoring adaptation before the next one began. Some pairs of jars being nearer together than others, the hands had to move farther in comparing, say, 1-5 than 1-2. This more extensive range would, of course, have greater cooling effect, even though the time of exposure to the air was alike in both cases. To obviate this, the hands were kept moving, while in the air, at about uniform rate for the whole two seconds, whether the distance between the jars was large or small.

Care was used always to immerse the hands to the same depth when making comparisons, because, as well known, stimulation of a larger area often gives the illusion of greater intensity of stimulus. There is here the additional factor that if the hand be inadvertently stimulated above the level of adaptation, the unadapted area often yields a distinctly stronger warm or cool sensation than the adapted part, and thereby judgment is confused. With practice the depth of immersion was kept uniform and accurate; and while, in a few individual cases, the judgment may well have been disturbed by too deep immersion, our method of statistical treatment presupposes variable errors of this type and is designed to care for them.

[17] Cf. article IX in the writer's Studies (*7b*) for ampler treatment of this point.

CHAPTER V.

Statistical Treatment of Data

1. *Derivation of Urban's Formula*

The primary data were treated according to Urban's modification of the psychometric function, $\Phi(\gamma)$. The essential features of this method will here be briefly given.[1]

Let S and J be two stimuli, the standard and the one to be judged, presented to an observer under specified conditions; let O respond to J, upon each presentation, in one of three ways (for example, with one of the judgments Warmer, Doubtful, Cooler); let p_w be the percentage of W judgments out of the total number given with a certain J. This method now makes two assumptions: 1) that p_w is some (determinable) function of the corresponding value of J; and 2) that their relation takes the form

$$p_w = \frac{\int_{-\infty}^{J} e^{-h^2 (x-a)^2}\, dx}{\int_{-\infty}^{+\infty} e^{-h^2 (x-a)^2}\, dx}$$

The denominator here equals $\frac{\sqrt{\pi}}{h}$ [for proof, see such

[1] The original presentation, in extended form, will be found in the article by Urban (*25*, XV, 261-355, and XVI, 168-227; especially XVI, 177-227). A critical analysis of certain elements of the method appears in Wirth *(32)*, in Thomson's articles (22 and 22a), and in Brown and Thomson *(6)*. The technique of using the method is well given by Boring *(5)*.

In view of the above presentations, the reader may well ask why another be added here. Our aim in this chapter is to present the method in a way at once brief and systematic, rigorous and clear. Urban's original treatment is very long and much beset with material which breaks the course of the argument; the same is true of Wirth's involved presentation; while Thomson sketches in those features which pertain to his own discussion, implying meanwhile a general knowledge of the method. None of the three, to all of whom the writer owes much, takes the trouble to set clearly forth the nature and range of the method's implications in the several stages of its development; and while they, of course, are fully aware of these inherent limitations, it is equally clear that many who use the method in practise are not. We here attempt to show, therefore, at appropriate stages of the exposition, just what conditions are presumed and must be instated if the method is to be rigorously accurate; and while these conditions are not easily attainable in the vicissitudes of actual experimentation and were by no means always achieved in our own study, it is at least helpful to know where the springs of error rise.

books as Weld (*30*, 180 f.) or Wilson (*31*, 364)]. The equation then becomes

$$p_w = (1/\sqrt{\pi}) \int_{-\infty}^{J} e^{-h^2 (x-a)^2} \, h dx.$$

Place $z = h(x - a)$. Since x ranges from $-\infty$ to J, z will range from $-\infty$ to $h(J - a)$, or

$$p_w = (1/\sqrt{\pi}) \int_{-\infty}^{h(J-a)} e^{-z^2} \, dz$$

When $J = a$, $h(J - a) = 0$ and $p = 1/2$ (since the integral from $-\infty$ to $+\infty$ equals unity). a therefore is that value of J for which the function p equals 1/2; in other words, the median. The median is defined, in this method, as the limen, the principal value which the equation yields. h is known as the "measure of precision" or, in terms of the standard deviation, $h = \frac{1}{\sigma\sqrt{2}}$. $J - a$, the distance from variable to median, is commonly called x. $hx = \frac{x}{\sigma\sqrt{2}} = \gamma$; and the equation becomes

$$p_w = 1/2 + (1/\sqrt{\pi}) \int_{0}^{\gamma} e^{-z^2} \, dz = 1/2 + 1/2\,\Phi(\gamma), \text{ where } \Phi(\gamma) = (1/\sqrt{\pi}) \int_{-\gamma}^{+\gamma} e^{-z^2} \, dz.$$

This being the final reduction, the method is usually known as the "Phi function of gamma" method.[2]

Our equation thus contains two unknowns, h and a; a being the median or limen, h being the precision of the distribution, from which the s.d. or p.e. of the limen may be determined. Given two values of gamma (γ_1 and γ_2), derived from two values of p (p_1 and p_2), for two values of J (J_1 and J_2), it is obvious that h and a would be uniquely determined from the two equations:

$$\gamma_1 = h(J_1 - a).$$
$$\gamma_2 = h(J_2 - a).$$

Since, however, it is desirable to have determinations of p for more than two values of J, we must calculate the most probable value of h and a by the usual method of least squares.

For this purpose the equations must be in linear form; but the relation between p, the observed value, and the unknowns, h and a, involves the transcendental function $\Phi(\gamma)$. The reduction to linear form may be directly accomplished in the following way (cf. Wirth, *32*, 136 ff. and 205 ff.): approxi-

[2] Special tables giving $\Phi(\gamma)$ for the corresponding values of γ are found in certain works on the theory of probability, such as Bruns (*7a*, *Anhang*). But $\Phi(\gamma)$ can also be determined with ease from customary tables of the probability integral, by means of the relation between γ and σ noted above.

mate values for h and a may be found by solving any two of the observation equations, or simple combinations of them (p. 205). Let these approximate values be h′ and a′; h′ may be treated as equal to the most probable value, h, plus some correction (presumably small), d; likewise a′ equals a plus some correction e. Substituting these values, we have

$$p = \tfrac{1}{2} + \tfrac{1}{2}\Phi(\gamma) = \tfrac{1}{2} + \tfrac{1}{2}\Phi[h\ (J - a)\ ] = \tfrac{1}{2} + \tfrac{1}{2}\Phi\ [(h' - d)\ \ (J - a' + e)]. \qquad (10)$$

Expanding the final form into a Taylor series in powers of d and e, we get[3]

$$\Phi[(h' - d)\ \ (J - a' + e)] = \Phi[h'\ (J - a')] - d\frac{\delta\Phi}{\delta h'} - e\ \frac{\delta\Phi}{\delta a'} + \frac{1}{2!}\left(d^2\frac{\delta^2\Phi}{\delta h'^2} + 2de\frac{\delta^2\Phi}{\delta h'\delta a'} + e^2\frac{\delta^2\Phi}{\delta a'^2}\right) + \cdots \qquad (11)$$

Only the first power of d and e can be used, since the equation is to be linear. The approximation thus secured will, however, be adequate in case d and e are small; otherwise the process must be repeated.

The above "Korrektionsverfahren" of Müller and Fechner is, however, less convenient in practise than the "Gewichtsverfahren" first developed by Müller. As before, we start with a series of equations

$$\gamma_k = h\ (J_k - a) \qquad (k = 1, 2, \ldots, n)$$

We must now secure the most probable values of h and a; such values that the mean-square deviation of the observed from the calculated p's will be a minimum. In other words, the mean-square adjustment can properly be applied only to directly observed quantities, such as p, and not to derived values like γ. In order to use our set of linear equations in γ, therefore, we must determine a weight, Ψ, such that

$$\Psi\ (\gamma - \gamma_o)^2 = (p - p_o)^2;$$

where p_o denotes the "most probable" value of p, that is, the adjusted value which corresponds to the most probable values of h and a; while γ_o is the value of γ directly derived from p_o. It is further obvious that this relation must eventuate:

$$\sum_n^1 \Psi\ (\gamma - \gamma_o)^2 = \sum_n^1\ (p - p_o)^2 = \text{a minimum.}$$

Let $t = \gamma - \gamma_o$. Now the adjusted value of p, namely,

[3] δ here denotes partial differentiation.

$$p_o = 1/2 + 1/2\ \Phi\ (\gamma_o);$$

the observed value, namely,

$$p = 1/2 + 1/2\ \Phi\ (\gamma);$$

hence,

$$\begin{aligned} p - p_o &= 1/2\ \Phi\ (\gamma) - 1/2\ \Phi\ (\gamma_o) \\ &= 1/2\ \Phi\ (\gamma) - 1/2\ \Phi\ (\gamma - t). \end{aligned}$$

Expanding $1/2\ \Phi\ (\gamma - t)$ into a Taylor series in powers of t, we have

$$1/2\ [\Phi(\gamma) - t\ \Phi'\ (\gamma) + (1/2!)\ t^2\ \Phi''\ (\gamma) - (1/3!)\ t^3\ \Phi'''\ (\gamma) + \ldots],$$

Deducting this quantity from $1/2\ \Phi\ (\gamma)$ and performing the differentiations indicated, we get

$$(p - p_o) = \frac{1}{\sqrt{\pi}}\ [t\,e^{-\gamma^2} + t^2\gamma e^{-\gamma^2} - 1/3\ t^3 e^{-\gamma^2}\ (1 - 2\gamma^2) - \ldots]$$

In general, the larger the number of terms in the right-hand member, the nearer will its value approach that of $(p_o - p)$. By employing the $\Phi\ (\gamma)$ method, however, we have already assumed that the series, p_1, p_2, .., p_n, forms an approximately Gaussian ogive. When this postulate is true, the value of $(p_o - p)$ is small, and likewise t is so small that its high powers may be neglected in the above series. Müller and Urban, in fact, retain only the first power of t, so that

$$(p - p_o)^2 = (1/\pi)t^2\,e^{-2\gamma^2},$$

and it is from this equation that their tabulated weights are derived. When the deviations (t) are small, these weights are accurate enough; but otherwise, the Urban P's should be used only in case a wide margin of error is tolerable. We may, however, dispense with any finer approximation because in adopting the present method we have already assumed the differences $(p_o - p)$ to be small; the tabulated P are accordingly used throughout in this study.[4]

The Urban "P" comprises an additional factor, 1/pq, which differentiates it from the original Müller weight. The percentage, p, which we obtain with a given value of J, is assumed to be itself merely a sample from a normal distribution; that is to say, if we make an indefinite number of trials at the same point (say, a thousand sets of 100 each when $J = 32.10°$ and $S = 32.00°$), the thousand values of p thus derived should form a closely normal distribution about their

[4] On the reliability of the Urban weights, P, see article V of the writer's "Studies in psychometric theory" (*7b*).

mean. The sequence of percentages turned out by a practised observer under uniform conditions (p_1, p_2, .., p_n) is thus presumed to be a true Bernoullian series, the elements conditioning the appearance and nonappearance of a given response being *constant* throughout (cf. Fisher: *12,* 118 ff.). The distribution has a standard deviation of $\sqrt{pq/n}$, q being $(1 - p)$ and n the number of trials from which p is derived, 100 in our example. If n be constant, the s.d. of these distributions for the several values of J will obviously not be constant, since pq varies. This product is maximal when $p = q = 1/2$ and decreases to zero when p or q equals zero. In fitting the most probable ogive to these percentages, each p is weighted, in the customary manner, inversely proportional to the square of sigma. Each p, therefore, weighs n/pq, which is minimal at $p = \frac{1}{2}$ and infinite when $p = 0$ or 1. Since the factor $e^{-2\gamma^2}$, however, decreases more rapidly than n/pq increases, the total weight is maximal at $p = 1/2$ and becomes zero at $p = 0$ or 1 (cf. Urban, *25,* XVI, 184).

These two, the Bernoullian and Müller's, are combined in the final Urban weight

$$P = (n/\pi)\ (1/pq)\ e^{-2\gamma^2},$$

which, as above noted, is tabulated in the more convenient form,

$$P = (1/4pq)\ e^{-2\gamma^2}$$

If the number of judgments for each value of J be constant, the factor n may be omitted, as in the table; when n varies from one set to another, as in our study, it must be included in the weight.

We are now ready to derive the normal equations for h and a from the s empirical ones. Given

$\gamma_1 = h(J_1 - a)$, with the weight P_1,
$\gamma_2 = h(J_2 - a)$, with the weight P_2,
. . . . . .
$\gamma_s = h(J_s - a)$, with the weight P_s.

Multiplying each equation by its weight and by the coefficient of h, and summing, we have for the normal equation in h,

$$[PJ\gamma] = [PJJ]\,h - [PJ]\,ah.$$

The equation in (ah) is

$$[P\gamma] = [PJ]\,h - [P]\,ah.$$

Solving simultaneously, we have

$$h = \frac{[P]\,[PJ\gamma] - [P\gamma]\,[PJ]}{[P]\,[PJJ] - [PJ]\,[PJ]},$$

$$a = \frac{[PJ]\,h - [P\gamma]}{[P]\,h}.$$

2. *Probable Error of the Limen*

The probable errors of h and a are still to be determined. Urban, in his monograph, computed the p.e.'s of the upper and lower limens and of the intervals between them—the "intervals of uncertainty"—and obtained values which, in every case, were larger, by as much as 10 times, than the quantities whose deviations they were (cf. Urban, *25,* XVI, 225 ff. and 192). Meanwhile Thomson (*22,* 52) has shown how inaccurate his figures are; and in lieu of Urban's formula, has developed another by means of the customary generalized procedure for the propagation of error in the case of *any* function of independently observed quantities (cf. Merriman, *16,* 78 f.).[5]

The p.e. of both h and a may, however, be found directly from h itself. This quantity is the "measure of precision" of the differential (normal) curve whose integral is fitted to the observed percentages. The s.d. of this distribution is $1/(h\sqrt{2})$, whence the p.e. of the median, a, is $.84535[1/(h\sqrt{2n})]$, and of h itself is $.6745[h/\sqrt{2n}]$.

We have now to choose between the Thomson formula, which gives an approximate p.e. for the exact (observed) values of p; and the h-procedure, which gives the exact p.e. of the approximate (adjusted) values of p. Neither, of course, is wholly reliable: our h-method, because it shows the deviation for the "most probable" smooth (normal) curve instead of for the empirical values of p; and Thomson's, for at least two reasons: (1) because of the abbreviated p.e.-formula which he uses; and (2), what is doubtless more important, because of the merely approximate values of P (the Urban weights), which appear again and again (some 11 times) in his equation.

Either method, of course, is really trustworthy only when the adjusted p's differ but little from the observed; but this we have already assumed by treating our data with Urban's

[5] This topic is developed more fully in article VI of the writer's Studies (*7b*).

function, Φ (γ). Which is more exact may well vary from one case to another; but in any event, the Thomson-formula has no discernible advantage to overweigh the inordinate labor which its computation involves;[6] we have therefore employed the h-method throughout in the present study.[7]

Since the p.e. of a varies inversely with the square-root of its weight, n, that is, the area of the normal curve fitted to the observed p's, the question remains: What is the value of n when the number of judgments upon which the different p's are based is not the same? When each p represents a certain percentage of the same number of judgments (450 in the example from Urban used above), then the total area of the normal curve (n) obviously is this constant number (450). When, however, the p's are not based on the same number of judgments, as in our study, what must we assume for the area (n) of the fitted curve? It was found by this formula:

$$\frac{\Sigma P_k n_k}{\Sigma P_k} = n_m \quad (k = 1, 2, \ldots, s)$$

where n_m is the weighted mean value of n. As an illustration of the procedure, consider the following series of *W* judgments where the standard temperature equals 20.00°:[8]

J	19.85°	19.90°	19.95°	20.00°	20.05°	20.10°	20.15°
p	.09	.29	.44	.51	.69	.69	.90
n	11	89	107	85	110	59	20
P	.51	.89	.99	1.00	.91	.91	.54

The sum of $P_k n_k$ here equals 440.55; the sum of P_k equals 5.75; therefore, n_m equals 76.59. In other words, if 76.59 judgments had been taken in each of the seven series above,

[6] One p.e. may be computed from h in about two minutes; by Thomson's method, if the writer may judge from his limited experience with it, something like four hours of intensive labor, with every mechanical aid for rapid work, are consumed in finding a single p.e. in a case like the above. When several hundred values have to be found, as in our study, the difference in time is of no little practical importance.

[7] The reader will note that both the Thomson- and the h-methods yield the p.e. of the crude (uncorrected) limen, which extends from S to that value of J where p = .50. The variability of the true limen, measured from Xi, should be nearly the same though not, of course, identical; but inasmuch as we know of no method for determining the p.e. of the true threshold, we have for the present to get along with that of the gross Urban limen.

[8] J is the stimulus to which the judgment, as given, applies; p is the percentage of Warmer judgments out of the total number (n) taken for that value of J; P is the Urban weight.

the p's remaining as they now are, the whole psychometric curve would also have turned out precisely as it now is.[9]

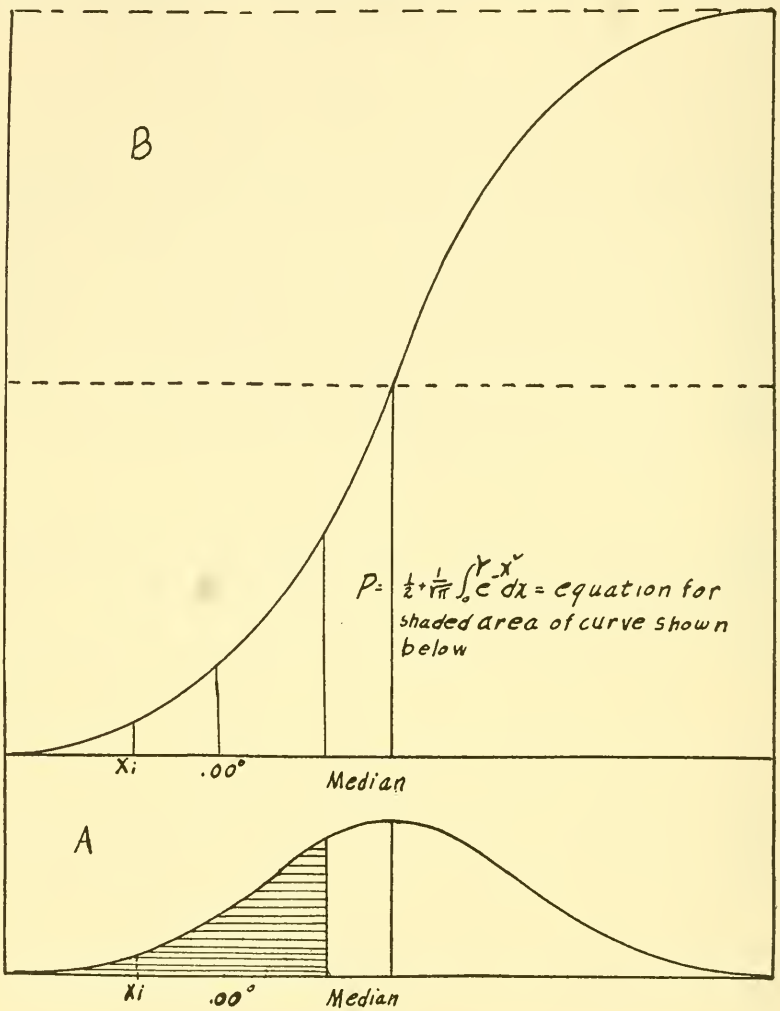


Fig. 1

The philosophy of the $\Phi(\gamma)$ method may be more apparent from the above cut. The curve A shows a "normal" Gaussian distribution, while B is the integral of A; that is, any ordinate of B is equal to the area of A from negative infinity up to that point (in the cut, the ordinate p, in B, is numerically equal to the shaded area in A); the equation for the two is identical, being

[9] The above treatment has been wholly theoretical; for a convenient summary giving all practical details of using the method, checking the results and so on, the reader may consult Boring's article (*5*).

$$p = 1/2 + (1/\sqrt{\pi})\int_{\circ}^{\gamma} e^{-x^2}\,dx.$$

The distance from the lower to the upper asymptote of B is unity, which is likewise the total area of A.

Given a series of Warmer judgments in percentage-frequencies, such as the following (S = 24.00°):

J	=	23.85	23.90	23.95	24.00	24.05	24.10	24.15
p_w	=	.00	.16	.48	.64	.85	.96	1.00

The curve B is the "most probable" Gaussian ogive fitted to such a series of ordinates. The crude limen is the distance from .00° to the median (where p_w equals .50) and may be larger or smaller than the "true" limen, which extends from Xi to the median. The value of h or σ for the two curves is the same.

CHAPTER VI.

Presentation of the Data

1. Quantitative Analysis

a. Methods Used

The basic figures drawn from the original records are reproduced in the Appendix. The quantitative results for all observers are presented under two headings: (A) the minima perceptibilia,[1] or limens found when the receptive surface was adapted to the temperature of the standard stimulus; and (B) the minima distingibilia,[1] or limens found when adapted to skin-temperature (32°) throughout. Under these two the following methods appear:

A, 1 : "Constant stimulus-differences; successive exposure of both hands to S and J, S being used, in each case, for adaptation as well as for the standard stimulus."
A, 2 : "Constant stimulus-differences, method of equivalents; simultaneous exposure of one hand to S and of the other to J, S being used, in each case, for adaptation as well as for the standard stimulus."
A, 3 : "Identical with A, 1; except that only one hand (right or left) was used."
A, 11: "Variable stimulus-differences, method of limits; successive exposure of both hands to S and V; adaptation to temperature of S."
B, 2 : "Identical with A, 2; except that the hands are adapted to 32° throughout."
B, 11: "Identical with A, 11; except that the hands are adapted to 32° throughout."
B, 12: "Analogous to A, 2; variable stimulus-differences, method of limits; simultaneous exposure of one hand to S and of the other to V; adaptation to 32° throughout."

In the first part of this chapter we shall briefly exhibit the extensive experimental data and thereafter seek to discover and correlate whatever inductions they can be made to yield and justify.

[1] These terms (cf. Alrutz, 2, 262 ff) are here used only as convenient designations, with no implication whatever as to the nature of adaptation.

b. Data from observer Culler (E-O); constant stimulus-differences

i. Method A, 1.

These data form a part of the writer's observations and are the most complete and consistent in our study; the details of procedure having been given above (Chap. IV) will not be repeated here.

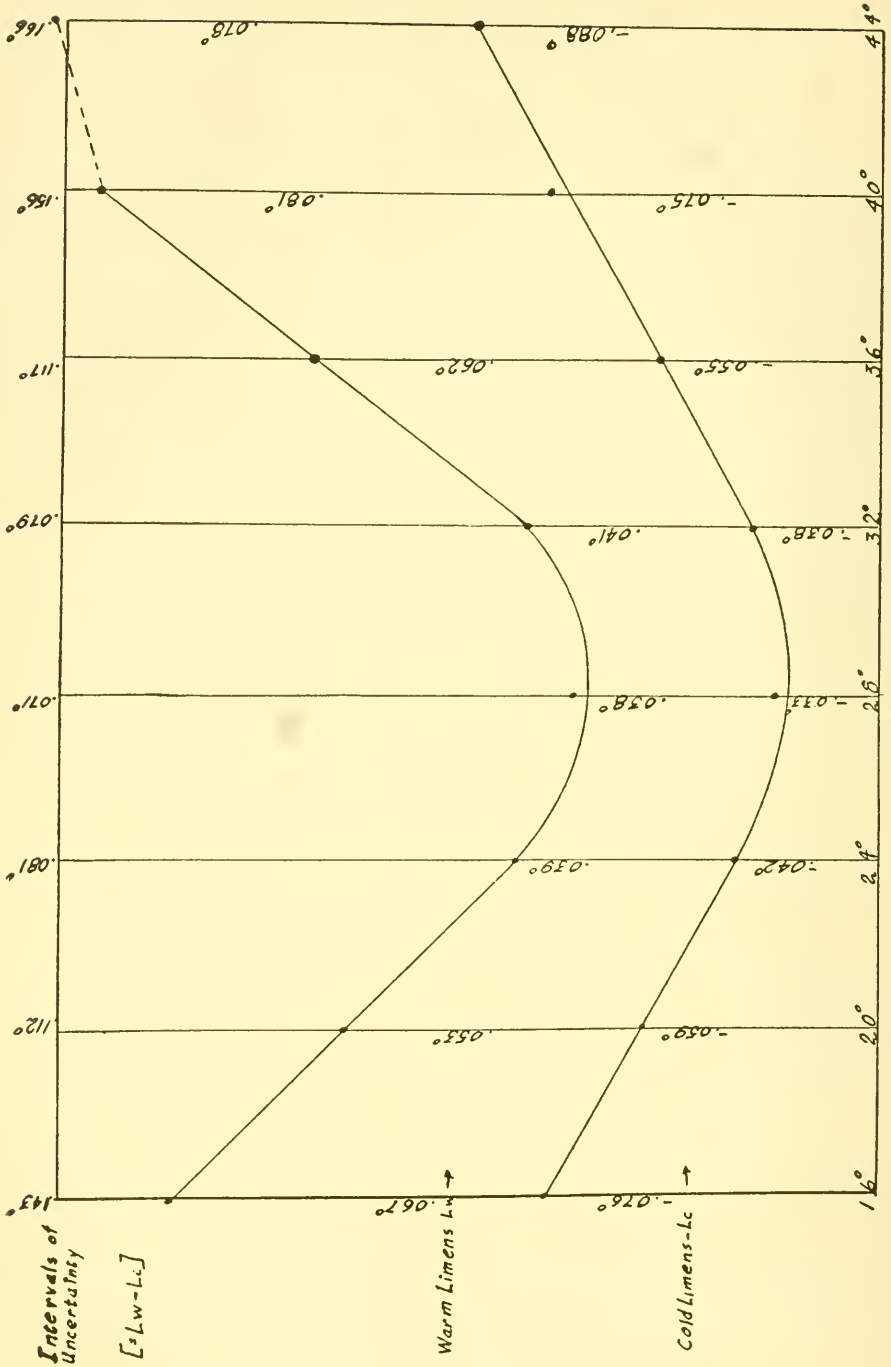
Consider Table VII and Figure 2, where the results are summarized in graphical form. The limens there given are not the figures which emerge directly upon solving the normal equations for $p_w = .50$ and $p_c = .50$, but are the true values, cleared of error. Any limen, as directly computed, is affected with a combination of constant tendencies (time-, space-, movement- and other errors). It has been commonly assumed that, upon combining the values for all the time-space orders, we secure a mean limen free of constant error. This is not the case; other tendencies (notably, expectation or habituation; see below, p. 62) still remain imbedded in the values and cannot thus be removed. We therefore in each case find the point *Xi*, where the judgments "warmer" and "colder" are equally probable and make this a "zero" from which both warm and cold limens are reckoned. The true warm limen, therefore, is the distance from the point Xi, where $p_w = p_c$, to the median point of the "warmer" ogive, where $p_w = .50$.

By what right do we adopt Xi as the subjective point of division between "warmer" and "colder"? Consider a series of figures like the following, where p_w and p_c denote percentages of warmer and colder responses:

S = 24.00°; METHOD A, 1.

J=	24.20	24.15	24.10	24.05	24.00	23.95	23.90	23.85
P_W=	1.00	.75	.58	.31	.18	.10	.00	.00
P_C=	.00	.06	.10	.34	.45	.62	.85	.92

Here $p_w = .50$ at 24.086° and $p_c = .50$ at 23.988°; the crude limens, measured from S=24.00° (L_w=.086°, L_c = —.012°), differ widely and are obviously much displaced by constant errors. But where shall we fix the subjective boundary between "warmer" and "colder"? "Colder" begins to appear



Method A1.
Fig. 2

TABLE VII.

MEAN VALUES OF L_w AND L_c FOR EACH TEMPERATURE COVERED BY METHOD A, 1.

	Group I						
	32°	28°	24°	20°	16°	20°	24°
L_w	.080	.076	.064	.048	.067	.059	.039
p.e.	.011	.013	.013	.010	.012	.010	.006
L_c	—.100	—.088	—.056	—.061	—.076	—.056	—.042
p.e.	.016	.015	.011	.013	.013	.010	.007
L_w-L_c	.180	.164	.120	.109	.143	.115	.081
p.e.	.019	.020	.017	.016	.017	.014	.009

	Group II				
	44°	40°	36°	32°	28°
L_w	.078	.081	.062	.041	.038
p.e.		.040	.008	.006	.006
L_c	—.088	—.075	—.055	—.038	—.033
p.e	.033	.055	.008	.006	.005
L_w-L_c	.166	.156	.117	.079	.071
p.e.		.068	.012	.008	.008

Notes to Table VII and Figure 2.

Each limen is the mean of the limens for the four time-space orders; see Appendix, p. 122. Each p.e. likewise is one-fourth of the p.e. of the sum of the four limens.

The table shows that, in Group I, we began at 32°, passing down by 4° intervals to 16° and then returning to 24°. We can thus determine, by comparing the first series with later ones at the same level, just where improvement with practise ceases. Consider these intervals ($L_w - L_c$):

	32°	28°	24°	20°
First	.180	.164	.120	.109
Second	.079	.071	.081	.115

At the first three levels each interval of the first series is much larger than in the later one; but at 20° the two are almost the same, the first being a trifle smaller. We therefore exclude the first three values from our graph; but from 20° on, we assume practise-improvement to have reached, and thereafter to maintain, its maximal level.

The value given for L_w at 44° is quite unreliable. Note the warm limens for the four time-space orders at 44° (see Appendix, p. 124):

	Limen	*p.e.*
S-J, R	.084	.053
S-J, L	.099	.055
J-S, R	.051	.040
J-S, L	.606	.679

All the p.e.'s are here comparable except the last, which is more than twelve times the size of any other; the limen is also on a different plane of magnitude than the remaining ones. The figure is without question wholly anomalous and may not properly be combined with the others. The mean of the remaining three (.078) is used as a rough approximation of the true value.

The Adjustment Equations.

(1) Limens from 16°-24° ($y = a + bx$; $x = 0$ at 24°).
(2) Limens from 32°-44° ($y = a + bx$; $x = 0$ at 32°).
(3) Limens from 24°-32° ($y = a + bx + cx^2$; $x = 0$ at 24°).

	a	*b*	*c*	*Weighted deviations of adjusted from observed values*			
(1) 16°-24°				16	20	24	
L_w	.0392°	—.0035°		.0001	—.0002	.0001	
L_c	—.0419°	.0043°		—.0001	.0004	—.0001	
L_w-L_c	.0811°	—.0078°		.0003	—.0006	.0003	
(2) 32°-44°				32	36	40	44
L_w	.0409°	.0053°		.0002	—.0003	.0001	
L_c	—.0383°	—.0042°		.0000	—.0001	.0001	.0000
L_w-L_c	.0792°	.0095°		.0002	—.0002	—.0002	
(3) 24°-32°				Minimal value of y			
L_w	.0391°	—.0006°	.0001°	.038° at 26.9°			
L_c	.0418°	—.0041°	.0005°	—.033° at 28.5°			
L_w-L_c	.0809°	—.0047°	.0006°	.071° at 28.2°			

A straight line ($y = a + bx$) is fitted to the limens from 16°-24° and 32°-44°; a quadratic curve with three unknowns to those from 24°-32°. In forming the normal equations, by the usual method of least squares, each limen is weighted inversely to its p.e.[2]; and in finding the weighted deviations of the adjusted from the observed values, this process is reversed so that each deviation is reduced to unit weight. The minimal values of y are found by equating the first derivative of the quadratic equation to zero and solving for x.

at 24.15°, which is far above 24.086°, while "warmer" continues down to 23.95°, which is well below 23.988°. Only one point, in our judgment, here stands out as the logical boundary; namely, where the tendencies to respond "warmer" and "colder" precisely balance, so that both are equally likely to appear. The true limens measured from Xi, which here falls at 24.037°, thus become .049° and —.049°. The methodological importance of Xi is referred to again; see p. 62.

Technical information on the Table and graph is given in the Notes (p. 59). A smooth curve fitted to the observed values resembles an hyperbola and might have been treated as such; instead, the data were divided into three groups for adjustment: 16°–24°, 24°–32°, 32°–44°. L_w at 44° being a mere approximation is not included in the equations. A straight line is laid through the limens for 16°-24° and 32°-44° and in every case fits the actual figures neatly; the weighted deviation never goes above 6 ten-thousandths. The interval 24°-32° not being linear, a quadratic curve is laid through it; the lowest point of this curve gives the absolute minimum perceptibile for the whole range of thermal stimuli; for the writer these minima are .038° in warm and .033° in cold.[2] That the human hand can accurately appreciate thermal differences of three to four hundredths degree is sufficiently

[2] These figures, though perhaps better than average, are by no means exceptional; the interval L_w-L_c at 32° for two other observers (method B, 1) is .078° and .084°, as compared with .081° for E-O (method A, 1).

surprising and reveals a capacity of the organism which perhaps few persons suspect.

The Weber Ratio in each case equals the coefficient of x, that is the slope of the fitted line; whence we derive the following *Weber Constants:*

	Warmer	*Colder*	*Interval*
16°-24°	.0035	.0043	.0078
32°-44°	.0053	.0042	.0095

These are the lowest fractions ever reported for any sense, but they are closely confirmed within the range 32°-44° by observers G and M (method A, 11; infra, p. 76). As to be expected, the slope-lines "round off" at the minimal point instead of meeting sharply at an angle; and thus confirm what is found in other fields, that the limens near the absolute minimum perceptibile are relatively too large.

The question still remains: what degree of *reliability* may we safely attach to these figures? This is found by the method shown in Table VIII. The difference of each limen from the one next to it is found along with the p.e. of the difference. We thus determine, in the usual way, the number of chances in a thousand that a difference of this size and direction is not due to "chance". We next divide the number of chances opposed to its validity by the number in its favor. If the differences, in size and direction, depended on chance alone,

TABLE VIII

COMBINED RELIABILITY OF THE SET OF INTERVALS (L_w-L_c)

					Diff.	*Validity of Diff.*		*Ratio: Against*
	I	*PE*I	*Diff.*	*PE*D	*PE*D	*For*	*Against*	*For*
40°	.156	.0680						
			.039	.0689	.56	.647	.353	.546
36°	.117	.0117						
			.038	.0145	2.63	.962	.038	.040
32°	.079	.0086						
			.008	.0120	.65	.670	.330	.493
28°	.071	.0085						
			.010	.0127	.77	.698	.302	.433
24°	.081	.0094						
			.031	.0141	2.20	.931	.069	.074
20°	.112	.0106						
			.031	.0204	1.50	.844	.156	.185
16°	.143	.0175						

The product of the six ratios in the last column = .000064, making less than 7 chances in 100,000 that differences of this size and uniformity of direction would have occurred by chance.

the product of these ratios should approximate unity; hence their actual product is a convenient index of the likelihood that they do result from chance. In L_w this product equals .0023, in L_c .00062, in the intervals .000064; we thus have less than 7 chances in 100,000 that the intervals would, by chance, have increased regularly on each side of the minimal value at 28° by the amounts here found. Further, the likelihood that these differences, in absolute value, should, by pure chance, so closely follow the well-defined uniformity which our values exhibit, is assuredly still less; but the writer knows no method of reducing this to quantitative terms. In any event, this evidence, supported by the fact that the Weber ratios for the range 32°-44° are closely confirmed by two other observers in method B, 1, may go to prove that our figures are reliable.

A matter of real consequence for psychometric method appears in Table IX, which gives the mean value of Xi for

TABLE IX.

28°	24°	20°	16°	20°	24°	44°	40°	36°	32°	28°
—.009	+.003	+.005	—.006	—.011	—.012	+.070	—.049	—.020	—.019	—.015
+.07	—.03	—.005	+.02	+.03	+.09	—.16	+.10	+.08	+.10	+.03

The second row of this table gives the mean of the four values of Xi (one for each time-space combination) at each temperature (method A, 1). The third row is taken from Table V (p. 31) and shows the percentage of "warmer" less the percentage of "colder" judgments. At 16° for example, .38 of all responses are "warmer" and .36 are "colder"; hence W-C = +.02.

each temperature and, directly beneath, the excess of warm over cold responses at the same level (from Table V, p. 31). Table V was compiled for the purpose of determining whether "warmer" and "colder" appeared about equally often in a given set of data; that is, whether the stimuli were "symmetrical" about the limen. A preponderance of "warmers", for example, might easily induce a tendency to give this response in doubtful cases, and thus artificially decrease L_w. We are now able to view the matter with new light from Xi. The table shows that, in *every* case, the sign of Xi is opposed to that of (W-C). At 16° the mean value of Xi is —.006°; this means that, apart from the time-space errors, "warmer" and "colder" are equally probable, not when S and J are the same, but when J is .006° lower than S. This shows that something is favoring the "warmers"; and therewith we find a predominance of warm-judgments (+.02). In *all* of the

11 cases, this opposed relation appears: when "warmer" is in excess, Xi falls below .00°; otherwise, it rises above .00°. The likelihood of this happening by chance is 1 in 2048 (2^{-11}); we may therefore assert with reasonable confidence that even when the stimuli are so arranged that the "warmers" and "colders" differ by only two or three per cent, some degree of error will still be induced. We can never hope to remove this error by trying to make our stimuli symmetrical about the limen; the relative number of warm and cold responses depends upon subjective tendencies which Xi alone can reveal and correct. Even though the crude limens be wrong, the position of Xi is a clear and convenient index of their degree of displacement. The usefulness of Xi is thus shown in revealing and removing a source of error which other methods do not reach at all.[3]

Because of variant space-time relations among the stimuli, any response, in the present method (A, 1), is affected with three forms of constant error: time, space and direction of movement (Table X). Given the four possible time-space combinations, corresponding to Müller's four Hauptfälle[4] (*18*, p. 64):

(a) S-J, left (J to the left and second)
(b) J-S, left (J to the left and first)
(c) S-J, right (J to the right and second)
(d) J-S, right (J to the right and first).

If we represent the several limens with L_a, L_b, and so on, the time-error [p] equals $(\frac{1}{4})$ $(L_a + L_c - L_b - L_d)$; the space-error [q] equals $(\frac{1}{4})$ $(L_c + L_d - L_a - L_b)$, and the movement-error [m] equals $(\frac{1}{4})$ $(L_b + L_c - L_a - L_d)$.[5] The three may be defined as follows: suppose the two stimuli, S and J, are equal; if the first one, whether right or left, seems warmer than the second, p is positive; if the one to the left, whether first or second seems warmer than the one to the right, q is positive; if J seems warmer when the second jar is to the left of the first, m is positive. In other words, if

[3] The methodological import of Xi is reviewed in article III of the writer's Studies (*7b*).

[4] p and q, of course, are the traditional Fechnerian time- and space-errors; the third, m, so far as we know, is here employed for the first time. Cf. Fechner (*11*, I, 113 ff), Müller (*18*, 64) and Titchener (*24*, d, 272).

[5] It may be noted that Müller's arrangement of the four combinations is not the same as Fechner's (*11*, I, 113).

the (gross) limen is larger: when J follows S, p is positive; when J is to right of S, q is positive; when the second stimulus-jar is to right of the first, m is positive. The error is negative, in each case, when the reverse is true.

TABLE X.

ERRORS OF TIME, SPACE AND DIRECTION OF MOVEMENT: METHOD A, 1.

		28°	24°	20°	16°	20°	24°
Time Error	Lw	—.041	—.027	.011	.049	.026	.025
	Lc	—.061	—.018	.020	.049	.016	.014
Space Error	Lw	.001	—.017	—.001	—.058	—.032	—.034
	Lc	.026	—.029	—.006	—.054	—.032	—.032
Movement Error	Lw	—.013	.012	.007	—.011	.001	—.001
	Lc	—.026	.010	—.004	.002	—.004	.004

		44°	40°	36°	32°	28°
Time Error	Lw	—.031	—.085	—.060	—.039	—.001
	Lc	—.071	—.105	—.071	—.030	—.003
Space Error	Lw	.080	.060	.028	—.001	.004
	Lc	.082	.054	.010	.018	.005
Movement Error	Lw	—.020	.016	—.006	.010	.004
	Lc	—.004	.025	—.016	.006	—.001

In these three are comprehended *all* the types of error which the purely time-space relations of S and J can possibly induce. We find not only the traditional two of Fechner, but a movement-error, here called m, as well, because the hand twice moves to the right in passing from first to second stimulus and twice to the left. It seems a bit curious that the classic treatments of psychophysic theory should make so much of the traditional errors of time and space, while they wholly pass over the third form, whose logical position and theoretical import are on precisely the same plane as the others. We can, to be sure, find hints or adumbrations, in the literature, of the movement-error, m; but our standard treatises fail to accept it as a necessary form of time-space error on a parity with p and q, where it belongs.[6]

The time-errors (apart from practise series) turn out to be uniformly positive at the lower levels (below 28°) and negative at the higher ones (above 28°). They are principally due to a disturbance of adaptation upon passing through the air from first to second stimulus. If the receptive surface is cooled in passage, the second stimulus appears warmer than it would otherwise be, and the error is positive. As might be foreseen, the figures are largest at the extreme

[6] See article I of the writer's Studies (*7b*).

levels, 16° on one side and 40°-44° on the other; they can not be referred, however, to the adaptation-level alone: 1) the air-temperature ranged from about 20° to 33°, while the data were being gathered, so that its effect varied greatly from time to time;[7] 2) this disturbance from the air is presumably not the sole root of the error.

If we sum the time-errors from 16°-28° and 28°-44°, both inclusive, we get, omitting practise series:

	28° down	28° up
L_w	.110°	—.325°
L_c	.096°	—.420°

The two limens are not equally affected by this type of error; the L_w is more disturbed by a positive one and the L_c by a negative. From 28° to 44° discrimination for cold is appreciably finer than for warm; but cooling of the receptive area interferes distinctly more with L_c than with L_w; below 28°, on the other hand, where the hands are but little cooled or even slightly warmed in the air, the warm limen is more disturbed by the error. A shift in the level of adaptation, therefore, alters discrimination primarily in the same direction.

Consider the time-error at 40°, where paradoxical cold is just beginning to emerge, and at 44°, where it comes to full effect: for both L_w and L_c the figures are markedly smaller at 44°, in spite of the greater discrepancy between adaptation- and air- temperature. Whatever the nature of adaptation, therefore, we see that when a receptor is "adapted" to paradoxical cold it is much less affected by normal cold stimulation as well.

The space-errors we find to be positive from 28° up and negative from 28° down with curious uniformity; prima facie, there is no apparent reason why the sign of the error should depend upon the level of adaptation; no causal relation seems to unite the two facts. But upon comparing these with the corresponding errors by method A, 2 (p. 70), we find a second curiosity: that the two series exhibit a markedly similar trend. The left hand's greater sensitivity to both warm and cold impressions accounts for the situation in A, 2; and we are constrained, in our opinion, to adopt a similar ex-

[7] To this we may ascribe the marked variance of the initial time-errors at 28° and 24° from the later ones; in the former case the room-temperature ranged about 20°-22°, in the latter about 26°-32°.

planation here: the judgment is largely determined by impressions from the left hand when J is to the left, and from the right hand when to the right. Two reasons may be offered for this: (1) The jars on the table were from one to six feet apart. In passing from first to second therefore the hands were often extended as far as possible, first in one direction, then in the other. The left hand could thus be more fully and readily immerged, when reaching to the left, whereas the right hand was often a little cramped and strained; the reverse was true on the right. Hence it may well be that a tendency arose, unobserved by E-O, to estimate both left and right stimulus primarily with the corresponding hand. (2) The distribution of attention may have favored the left hand when on that side, either through a kind of unconscious association, or because method A, 2 (simultaneous exposure of left hand in left, and right hand in right, jar) alternated with the present one. In any event, our solution is confirmed by an additional fact: that the amount of error varies closely with the intensity of stimulation, being largest at the extremes 16° and 44° and least around 28°. The difference of sensitivity would naturally induce larger errors at higher stimulus-intensities.—The matter well exemplifies the growth and persistence of an unobserved tendency.

There are no large or consistent differences between L_w and L_c in this error, whether from 28° up or from 28° down.

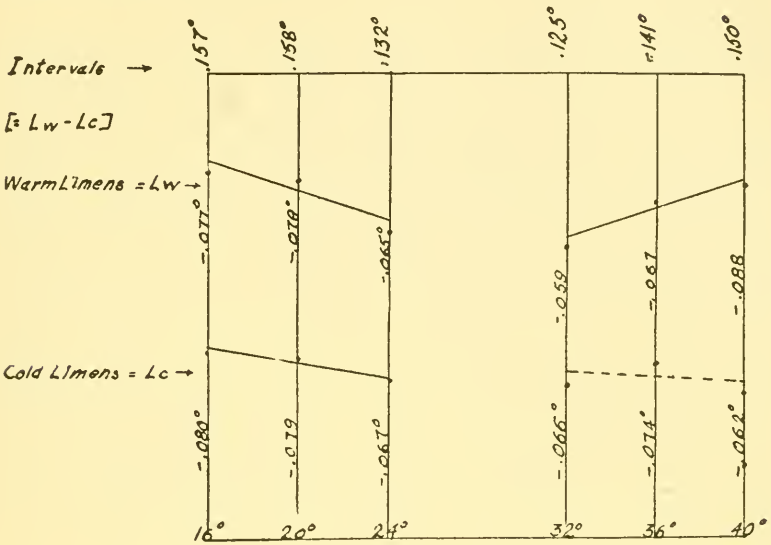
The movement-errors, finally, reveal no uniformity in size or direction: in L_w 5 in 11 are negative, in L_c 6 are negative. While they are practically unimportant in this case, they are theoretically on a par with the other two.

ii. Method A, 2.

By this method, the hands upon leaving the adaptation-jar were simultaneously immersed, after a two-second interval, the right hand in the right, and the left hand in the left, jar. The judgment invariably applies to J. Other details of procedure are as given under method A, 1 above.

Consider Table XI and Figure 3, where the results are summarized in graphical form. As in method A, 1, the limens are reckoned in each case from Xi, the point of subjective equality, where the responses warmer and colder are equally likely to appear (see p. 57 for theory of this procedure).

Technical information on both table and graph is given in the Notes (p. 68).



Method A 2

Fig. 3

TABLE XI.

MEAN VALUES OF L_w AND L_c FOR EACH TEMPERATURE COVERED IN METHOD A, 2.

Group I

	28°	24°	20°	16°	20°	24°
L_w	.116	.095	.073	.077	.082	.065
p.e.	.018	.018	.017	.032	.012	.016
L_c	—.109	—.094	—.069	—.080	—.087	—.067
p.e.	.016	.020	.016	.030	.014	.016
L_w-L_c	.225	.189	.142	.157	.169	.132
p.e.	.024	.027	.024	.044	.019	.023

Group II

	40°	36°	32°	28°
L_w	.088	.068	.059	.136
p.e.	.020	.013	.010	.033
L_c	—.062	—.074	—.066	—.108
p.e.	.012	.015	.011	.026
L_w-L_c	.150	.142	.125	.244
p.e.	.024	.020	.014	.042

Notes to Table XI and Figure 3.

Each limen as given is the mean of the limens for the two space orders; see Appendix, p. 125. Each p.e. likewise is one-half of the p.e. of the sum of the two limens.

We find, as in A, 1 (p. 59), that improvement with practise ceases with the first 24°; hence all values thereafter are used in the graph and adjustment equations.

The figures for 28° ($L_w = .136$, $L_c = -.108$) are quite anomalous and cannot be used with the others, for three reasons: (1) The figures are much higher than any others in the whole range (16°-40°), being about twice as large as those for 24° and 32°, adjoining; whereas the course of the fitted curves would lead us to expect a smaller value at 28°, as in method A, 1, than at either 24° or 32°. (2) These figures for 28° run higher than those for the initial 28°, at the very beginning of practise. (3) The p.e.'s are two or three times as large as those for the adjoining values. We thus seem justified in rejecting the figures as wholly unreliable; it is regrettable that we have no others to use in their stead.

The relative size of the warm and cold limens at 40° is also open to question for two reasons: (1) The increase in L_c from 32° to 36°, the slope of the intervals (L_w-L_c) from 32° to 40° and the course of the corresponding values by methods A, 1 and B, 1 would, all three, lead us to expect a larger L_c at 40° than at 36°; on the contrary it proves to be noticeably smaller, while L_w seems excessively large. (2) Throughout the rest of the graph L_c equals or exceeds L_w, whereas here it falls far below it. The relative size of these two limens is therefore open to doubt; and the linear fit, shown by the path of the dotted line, has little significance.

The Adjustment Equations.

(1) Limens from 16°-24° ($y = a + bx$; $x = 0$ at 24°).
(2) Limens from 32°-40° ($y = a + bx$; $x = 0$ at 32°).

	Weighted			*Unweighted*		
	a	*b*	Σd^2	*a*	*b*	Σd^2
(1) 16°-24°						
L_w	.0677°	—.0023°	13	.0675°	—.0014°	24
L_c	—.0689°	.0022°	11	—.0685°	.0017°	14
L_w-L_c	.1366°	—.0045°	54	.1360°	—.0031°	74
(2) 32°-40°						
L_w	.0573°	.0034°	24	.0561°	.0038°	19
L_c	—.0685°	.0005°	41	—.0699°	.0006°	74
L_w-L_c	.1258°	.0029°	14	.1260°	.0032°	14

Under "Weighted", the equation for the fitted line is found by weighting each limen inversely to its p.e.^{2}, as in method A, 1; while in finding the sum of the squared deviations (Σd^2) of the adjusted from the observed values, this process is reversed so that each deviation is reduced to unit weight. The figures under d^2 show millionths of a degree. In the "Unweighted" equations each limen carries the same weight. The two sets give somewhat different slopes to the fitted lines, that is, different values for the Weber Constants.

A straight line, as in A, 1, is laid through the limens on each side of 28°, but in no case fits the values neatly, which show a small, but apparently real, tendency to negative acceleration; a quadratic adjustment for the same figures is given below (p. 103). The slope of the lines, that is, the Weber con-

stants by this method have therefore a merely nominal significance; it will be seen that the Weber ratio (= b, the coefficient of x) ranges in absolute value from .0029 to .0045 for the intervals L_w-L_c, by either method of calculation, weighted or unweighted; these values are about half the corresponding figures (.0078 and .0095) by method A, 1 (p. 61).

We thus find three outstanding differences between these curves and those in A, 1: (1) on the whole, the limens are considerably larger; (2) their mean slope is much smaller; (3) the curves are negatively accelerated instead of rectilinear.

What is the explanation and import of these differences? Let us consider them in order. (1) This seems due to a disparity of thermal sensitivity in the two hands. Compare the following figures at 32°, for similar conditions:

	L_w-L_c	I.D.
Both hands (A, 1)	.180°	5.56
Left hand, alone	.186°	5.38
Right hand, alone	.394°	2.54
Mean, right and left		3.96
Hands separate (B, 2)	.256°	3.91

The index of discrimination (I.D.) is the reciprocal of the combined limens L_w-L_c; at 32°, we may note, method B, 2 coincides with our present method A, 2. Two facts appear from this table: (a) the discriminative index of both hands together (5.56) is almost identical (3.0% higher) with that of the more sensitive (left) alone (5.38); just as the visual acuity of the two eyes exceeds but little that of the better one alone; (b) the index of both hands used separately (3.91) is almost identical (1.3% less) with the mean of the indices for the two hands alone (3.96). Here fact and theory meet: comparison of the two stimuli must depend in equal measure upon impressions from the separate hands; whence the resulting discrimination will obviously fall about midway of the two contributing indices. The difference in method (simultaneous and successive presentation) seems to have but small effect. We should thus expect the present limens to run higher than those in A, 1 by (.256/.180) or (5.56/3.91); calculating we get .112 and .115 for L_w-L_c at 32° and 24° instead of the observed values .125 and .132. The agreement is not very good and shows that these data, along with other irregularities, run higher in the medial range than we should expect.

Points (2) and (3) are perhaps two sides of the same fact and will so be treated here. Whereas the present limens are about 60% higher than the corresponding values in A, 1 at 32° and 24°, at the extremes, 40° and 16°, they are about the same. When discrimination by A, 2 is so much poorer in the medial range, how does it come to be equally good at the extremes? What increase we find, furthermore, is not evenly distributed but takes the form of a negatively accelerated curve. On each side of 28° the curve seems to be approximating an upper limit beyond which it will not pass, just as each has a lower limit below which it cannot be depressed. As we attain certain magnitudes of stimulus or j.n.d. the limen seems no longer to increase at the previous rate but instead settles down to a limit. Not merely the relative size of the stimuli but their absolute intensity as well, the degree in which they depart from the ordinary range to which the organism is required to adjust, seems to play a role.

If the unlikeness of the two curves be genuine, the writer has no real explanation to propose; but the validity of their difference may be questioned. Almost every important value of A, 2 is but one-third to one-eighth as reliable as the correlative figure of A, 1; hence the disparity of the two curves may be due largely, or even wholly, to the high variability of A, 2.[8]

The space-errors are predominantly negative at the lower levels and positive at the higher ones (Table XII). The most obvious causal factor is the greater sensitivity of the left

TABLE XII.

SPACE-ERRORS. METHOD A, 2.

	Group I						*Group II*		
	28°	24°	20°	16°	20°	24°	40°	36°	32°
L_w	.017	.040	.030	—.051	—.068	—.035	.091	.061	.008
L_c	—.032	.029	.011	—.051	—.021	—.015	.099	.061	.012

hand, as noted above; at 16°, for example, if the left hand were in water .051° warmer than was the right one, the two impressions would be judged equally cool. At higher temperatures, likewise, the warmth has more effect upon the left hand than the right.

[8] This interpretation is virtually proved in article IX of the writer's Studies (*7b*).

If we sum the space-errors, disregarding sign, for 28°-44° and 24°-16° in all data by method of equivalents (A, 2; B, 2; B, 12), we get:

	28°-44°		24°-16°
		A, 2	
L_w	.177°		.224°
L_c	.204°		.127°
		B, 2	
L_w	.842°		
L_c	1.223°		
		B, 12	
L_w	1.314°	(Lively)	
L_c	1.369°		
L_w	.624°	(Kraeuter)	
L_c	.694°		

With E-O (methods A, 2 and B, 2) and Lively, the left hand is more responsive at all levels to both cold and warm stimulation, while with K the right is more sensitive. Now the table shows, in every case, that the more susceptible hand becomes proportionably more responsive to *cold* from 28° up, and from 24° down relatively more sensitive to *warm*. Upon passing from S to V in the range 28°-44°, however, the hands are cooled, whereas in 24°-16° they are, on the whole, warmed; whence it follows that cooling disturbs the cold limen, and warming the warm one, less in the more susceptible hand than in the other. That is, a shift of adaptive level, whether up or down, affects the correlative limen in the more responsive hand less than in the other.

iii. Method A, 3.

This method is in all respects identical with A, 1 above, except that only one hand (right or left) and only the one time-order (S-J) were used.

Readings were taken at 32° only, in the early stages of practise, for the purpose of comparing the sensitivity of the two hands. The limens cannot be separated, since only one time-order was used; but the intervals (L_w-L_c) are:

	I	p.e.
Left hand	.186°	.029
Right hand	.394°	.141

If discriminative capacity is inversely proportional to the magnitude of these intervals, the left hand proves to be 2.12 times as sensitive as the right; whether this is equally true

of warm and cold impressions cannot be said. The p.e.'s also reveal the greater precision of the left hand. We have already noted how these differing indices of discrimination for the two hands affect the liminal values and space-errors of methods A,1 and A,2.

iv. Method B, 2.

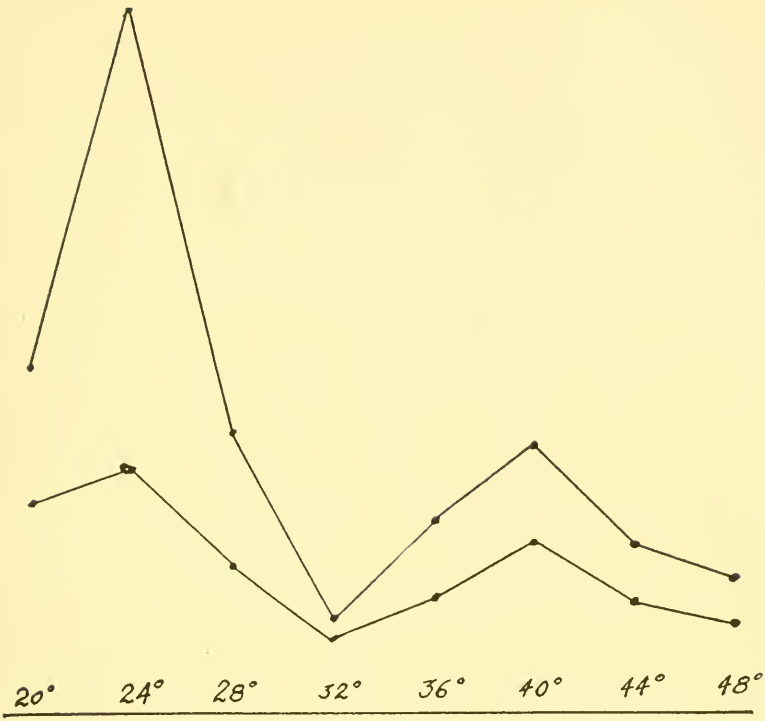
This method is in all respects identical with A,2, above, except that the hands were kept adapted to 32° in a separate adaptation-jar. The left hand was always immersed in the left stimulus-jar; when J was to the left of S, the space-order is left; and all judgments, as usual, apply to J.

All forms of the B method yield a minimum distingibile as against the minimum perceptibile of the A methods, with which we have dealt heretofore. Whatever the nature of adaptation and the resultant difference in these two types of minima, certain facts are apparent:

(1) The present limens *greatly exceed* the corresponding ones in A; their magnitude and their p.e.'s both prove how uncertain discrimination becomes when it proceeds at a temperature widely differing from that to which the sensory surface is adapted. Owing to limited number of observations and persistence of practise effects the figures, in a few cases, tend to run too high; but after allowing for these factors we are still on another plane of magnitude than in A,2, except at 32°, where the methods coincide.

(2) Of much greater moment is the course of the fitted curves. We find no resemblance to the nearly rectilinear increases of A; on the contrary, a strongly *negative curvature* appears in every case. The reality of this difference is abundantly confirmed by further data from other observers and methods. Whereas, therefore, in some conditions, a Weber ratio appears with notable constancy and accordance, in method B no approximation to it can be discerned. The matter will be further discussed in our general conclusions (p. 101).

(3) Of incidental interest are the small values at 48°, which is furthest removed from the adaptation-level. Pain here first becomes sufficiently intense to disturb the normal temperature-criteria; discrimination is no longer purely thermal and the values cannot be used in fitting the curves. This factor



Method B 2.

Fig. 4

TABLE XIII.

MEAN VALUES OF L_w AND L_c FOR ALL TEMPERATURES COVERED IN METHOD B, 2.

	Group I					*Group II*			
	32°	28°	24°	20°	48°	44°	40°	36°	32°
L_w	.113°	.386°	1.380°	.409°	.133°	.173°	.287°	.242°	.056°
p.e.	.015	.155	.453	.092	.030	.090	.118	.093	.012
L_c	—.142°	—.321°	—.606°	—.498°	—.113°	—.182°	—.381°	—.201°	—.078°
p.e.	.021	.128	.198	.111	.025	.093	.155	.081	.019
L_w-L_c	.255°	.707°	1.986°	.907°	.246°	.355°	.668°	.443°	.134°
p.e.	.026	.201	.494	.144	.039	.129	.194	.123	.023

Notes to Table XIII and Figure 4.

Each limen as given is the mean of the limens for the two space-orders (Appendix, p. 127); except that with the three temperatures below 32°, it was deemed advisable, owing to the relatively small number of cases and low precision, to combine the figures for the separate space-orders prior to computing the limen. In so doing the percentages of "warmer" and "colder" responses under Right and Left were equally weighted. Each p.e. likewise is one-half the p.e. of the sum of the two limens.

It is likely that practise-effects had not wholly ceased by 28° (cf. p. 59) and the figures at 24° are unquestionably excessive; but in default of other values they were used and weighted inversely to their p.e.[2] in finding the most probable curves, whence their relative influence upon the adjusted curve is proportionably slight. It will be seen that the curves for L_c from 32°-20° agree quite well with those from 32°-44°, which were obtained under maximal practise-efficiency.

The Adjustment Equations.

(1) Limens from 32°-20° ($y = a + bx + cx^2$; $x = 0$ at 32°, in all cases).
(2) Limens from 32°-44°

					Weighted deviations		
	a	*b*	*c*	32°	28°	24°	20°
1) 32°-20°							
L_w	.0554°	—.1630°	—.0110°	—.0023	.0072	—.0068	.0023
L_c	—.0777°	.0884°	.0044°	.0028	—.0071	.0067	—.0022
L_w-L_c	.1331°	—.2514°	—.0154°	—.0051	.0143	—.0135	.0045
2) 32°-44°				32°	36°	40°	44°
L_w	.0560°	.0649°	—.0046°	.0000	.0000	—.0002	.0002
L_c	—.0779°	—.0542°	.0037°	.0021	—.0071	.0071	—.0024
L_w-L_c	.1339°	.1191°	—.0083°	—.0021	.0071	—.0073	.0026

A quadratic curve is laid through the limens on each side of 32°. In forming the normal equations, by the usual method of least squares, each limen is weighted inversely to its p.e.[2]; and in finding the weighted deviations of the adjusted from the observed values, this process is reversed so that each deviation is reduced to unit weight.

appears again, when its variant effect upon different observers will be noted (p. 78).

The space-errors are given below and greatly exceed in absolute size the corresponding figures in A, 2 (p. 70), just as do the limens. The greater sensitivity of the left hand again induces large positive errors above 32° and negative ones below.

TABLE XIV.

Space-Errors. Method B, 2.

	32°	28°	48°	44°	40°	36°	32°
L_w	.032	—.139	.140	.275	.087	.250	—.059
L_c	.034	—.357	.119	.049	.362	.343	—.078

c. Data from other observers; method of Limits.

The following data were secured during one summer-session from four observers[9] and were, in a sense, preliminary to the

[9] These persons were: Misses Vitus Mulvey and Bertha Lively, students with good training in Psychology; Karl Kraeuter, who had some previous work in the subject; and Professor L. R. Geissler, who was of course the most competent observer in the group. To all these persons, and in especial degree to Dr. Geissler, the writer wishes to acknowledge his indebtedness.

material above presented. Three forms of the method of Limits (variable stimulus-differences) were used, known as A, 11, B, 11 and B, 12; they are defined on p. 56 above.

i. General Description of Procedure

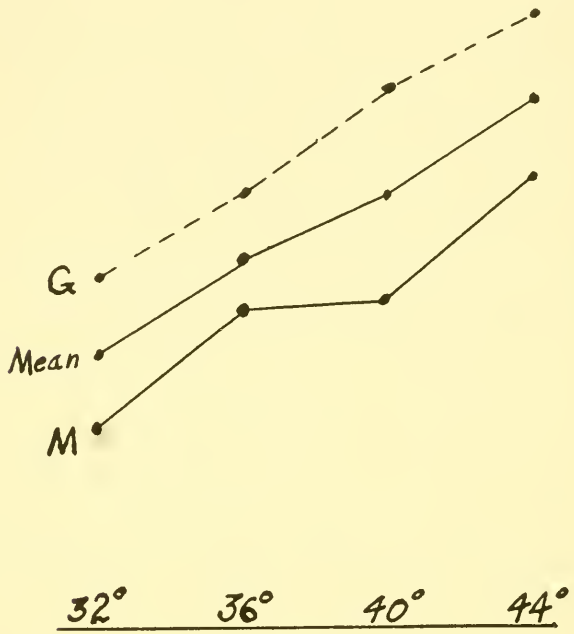
Five jars were used, the middle one being for adaptation to 32°. On its left was a standard (S) held constant at the temperature under investigation (32° to 48°); to the left of S was a variable jar (V). On the right of the middle jar was another S, with a V to its right. Both space-orders could thus be used, V to left and to right of S; the two standards were always held at the same level. The temperature of V was raised by means of a large electric heater-lamp and lowered by means of a container with melting ice so placed in the jar as not to disturb O's hands.

A descending series began with V distinctly warmer than S, passing down through a range of "equality" or "no difference" until the judgment "cooler" was given; the ascending series rose from distinctly "cooler" to "warmer." When going down, the point midway between the last "warmer" and the first "equal" was taken as the turning-point or warm limen for that series; likewise, the point midway between the last "equal" and the first "cooler" served as the cold limen. Regularly, in one hour, four series down and four up were taken, making eight determinations of each limen. With each mean in the tables is given the number of cases on which it is based.

The procedure in these trials was not so well standardized as in the constant methods (A) above. The time of exposure to each stimulus was about two seconds for each O, but was not regulated with a metronome. As in other methods, the time of adaptation ranged from three minutes at 32° to five minutes at the extremes (40°-44°). One trial was made and judgment given every twenty seconds. In methods A, 11 and B, 11, only one time-order was used, from S to V; the time-error is thus indeterminable and the true limens cannot be found; but the "equality" intervals, which correspond to the "intervals of uncertainty" (L_w-L_c) in our previous results, are available for comparative analysis. During or at the close of the working-period, the observer was invited to report anything which might corroborate or explain the objective results; practically all the verbal comments thus secured are from Geissler and Lively.

ii. Method A, 11; Observers Geissler and Mulvey

Here both hands, after being adapted to the temperature of S, were successively exposed, first to S, then to V; two observers, Geissler and Mulvey, covered the range from 32° to 44°. The series of values approximate a straight line, especially with G; and the slope of the fitted line, that is, the *Weber*



Method A II

Ratio turns out to be .0099 for G, .0087 for M, and .0093 for their mean. These figures rather strikingly confirm E-O's results by the analogous constant method (A, 1), where the slope of the corresponding intervals is .0095. Owing to the use of but one time-order, the intervals for G and M cannot be separated into their components, L_w and L_c; but that the slope of the interval which they comprise (especially with G, the most competent observer of the group) so closely accords with the figure found by another observer in another method, may be

taken as additional evidence of the reliability of these results. We should not expect the intervals to agree in actual size, of course, for the three observers differ in sensitivity and the methods are not the same.

The errors of space and expectation will be later discussed (p. 82).

iii. Method B, 11

This method differs from A, 11 (successive exposure of both hands, first to S, then to V) only in that the hands were previously adapted to 32° in all cases; the four observers (G, K, L, M) covered the range from 32° to 48°. The resulting values are shown in two groups on the basis of general resemblance (Fig. 6). Characteristic of those for G and M is a moderate increase and strongly negative curvature from

TABLE XV.

MEAN VALUES OF L_w-L_c: METHOD A, 11

	32°	36°	40°	44°
Geissler	.151	.186	.234	.267
m.d.	.005	.007	.010	.015
Mulvey	.084	.135	.140	.195
m.d.	.005	.010	.009	.009
Mean of G and M	.117	.160	.187	.231

Notes to Table XV and Figure 5.

The figures given are, in each case, the means of from 29 to 48 separate determinations; see Appendix, p. 129. To abridge the labor of calculation, the mean deviation was computed in each case in lieu of the p.e.

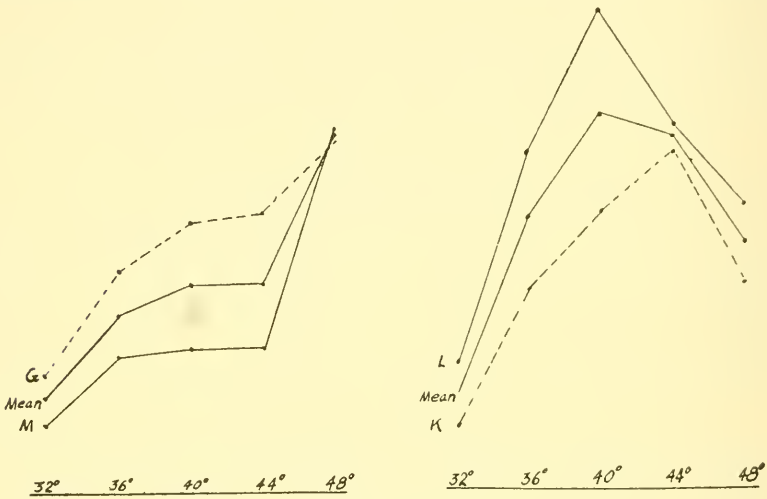
The adjustment equations.

Form of equation: $y = a + bx$; $x = 0$ at 32°.

	a	b	*Weighted Deviations* 32°	36°	40°	44°
G:	.1502°	.0099°	—.0018	.0036	—.0024	.0004
M:	.0847°	.0087°	.0015	—.0073	.0101	—.0042
Mean:	.1174°	.0093°	—.0003	—.0037	.0077	—.0038

Straight lines ($y = a + bx$) are laid through the observed values and fit them very well, especially for observer G; the deviations for G average about two thousandths, for M about six. In forming the normal equations, by the usual method of least squares, each figure is weighted inversely to its m.d.^{2}; and in finding the weighted deviations of the adjusted from the observed values, this process is reversed so that each deviation is reduced to unit weight.

36° to 44°, with a pronounced "jump" at 48°; in K and L, we find a steep initial rise, followed by a pronounced drop. The disagreement, which is so gross as to look very curious, may, in the writer's opinion, be explained as follows. At 48° pain is pretty intense; this was often mentioned by the observers. If it affects the process of judging at all, it will either hinder or help discrimination. Let us consider the two alternatives. (1) If O's temperature-criteria are well in hand, then the obtrusive emergence of other, secondary ones is likely to be confusing, at least for a time. Observer G reported, for example, on at least three days of the four at 48°, that pain



Method B II

served as *one*, or as *the*, criterion of judgment; whence his figure at 48° is no thermal limen at all and may not be compared with the preceding values at 32° to 44°. In the case of G (and M) the pain-intensities may have been intrinsically harder to discriminate, or harder simply from lack of practise and familiarity with them as criteria. Owing to the pain-quality, the hands may also have been immersed for a shorter time. In any event, the intrusion of the pain quality obviously displaced, in whole or in part, the thermal criteria, without supplying others of equal accuracy. (2) The pain-quality may be of aid in discrimination, on the contrary, if O's thermal criteria are not well-defined. This seemed to be specially true in the case of observer L. Her work was always erratic; she

was extremely conscientious, but never seemed to be quite sure of her criteria. For example, after having given several "equals" she would ask the recorder to change them to "warmer"; or in a down-series, after having changed from "warmer" to "equal", she would revert to "warmer" for several trials. To her the added quality of pain or stinging seemed to give an assurance thitherto lacking. She once reported that it

TABLE XVI.

MEAN VALUES OF L_w-L_c: METHOD B, 11.

	32°	36°	40°	44°	48°
Geissler	.151	.287	.353	.360	.460
m.d.	.005	.011	.020	.019	.026
Mulvey	.084	.176	.185	.184	.472
m.d.	.005	.013	.013	.012	.028
Kraeuter	.078	.262	.353	.437	.265
m.d.	.006	.021	.021	.042	.029
Lively	.162	.438	.629	.470	.367
m.d.	.008	.031	.052	.035	.030

Notes to Table XVI and Figure 6.

The figures given are, in each case, the means of from 16 to 48 separate determinations; see Appendix, p. 129. As in A, 11, the mean deviation is shown instead of the p.e. The values of 48°, where pain is very obtrusive, are shown, but not used in the adjustment equations.

The adjustment equations.

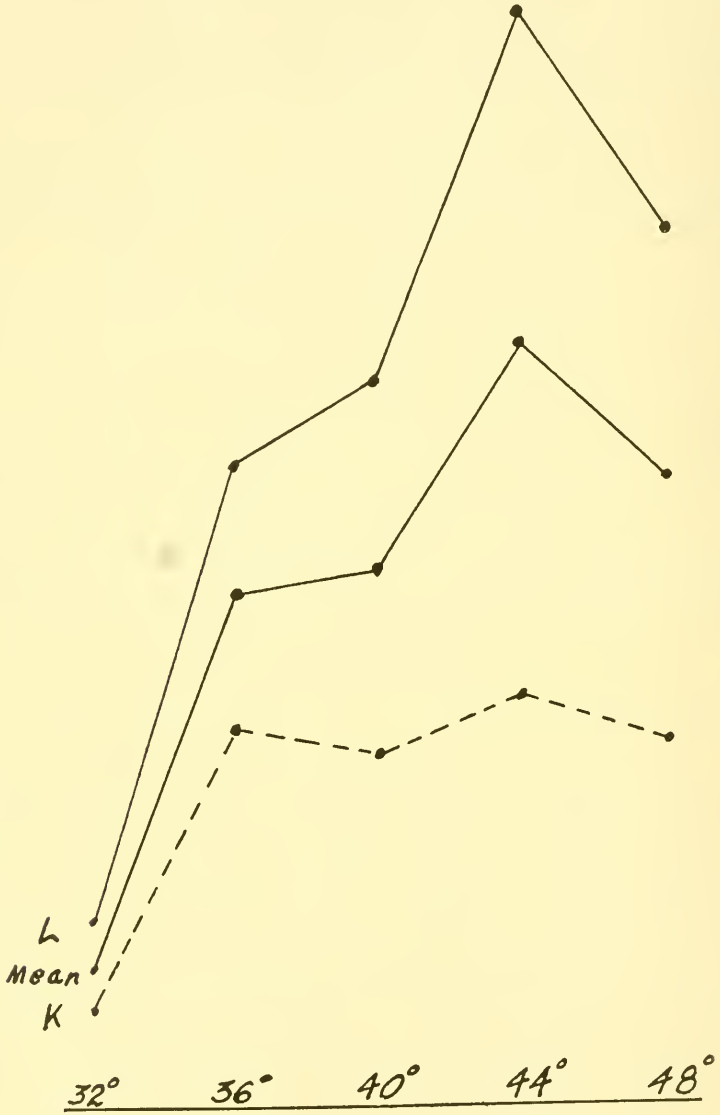
Type of equation: $y = a + bx + cx^2$; $x = 0$ at 32°.

	a	*b*	*c*	*Weighted deviations* 32°	36°	40°	44°
G:	.1510°	.0420°	—.0021°	.000	.000	.001	.000
M:	.0845°	.0260°	—.0015°	.001	—.005	.005	—.002
K:	.0783°	.0501°	—.0018°	.001	—.003	.003	—.001
L:	.1613°	.0993°	—.0061°	—.002	.006	—.006	.002

Quadratic curves ($y = a + bx + cx^2$; $x = 0$ at 32°) are laid through the observed values and fit them quite well, especially in the case of G. In forming the normal equations, by the usual method of least squares, each figure is weighted inversely to its m.d.^{2}; and in finding the weighted deviations of the adjusted from the observed values, this process is reversed so that each deviation is reduced to unit weight.

was "easy to tell the difference (at 48°); relies upon pain quality rather often". Again she tried one day to "disregard the stinging quality and judge *only* from temperature by not

keeping hands immerged long enough for the stinging to develop"; her mean equality-interval for that day increased about 60 per cent over its previous value (from .860° to



Method B 12.

1.372°).[10] In the case of L (and presumably of K), therefore, the pain-criteria were more refined than the thermal ones which they disturbed.

Owing to the pain-quality at 48°, only the values from 32° to 44° are used in fitting the curves. The path of the successive figures is, in every case, so clearly curvilinear that only a quadratic adjustment is shown. We here find the same phenomenon as appeared in the analogous methods with constant stimuli: under adaptation to S (A, 11), the limens increase uniformly and thus yield a true Weber Constant; with adaptation to 32°, on the contrary (B, 11), the negative curvature is so palpable and consistent that no hint of a constant ratio can be discerned. The dependence of Weber's Law upon adaptation thus appears to be unmistakable. The matter will be taken up again (p. 101).

As to be expected, the mean deviations in B, 11 (adaptation to 32°) run higher than in A, 11 (adaptation to S); compare the figures for G and M.

The errors of space and expectation will be treated below (p. 82).

TABLE XVII.

MEAN VALUES OF L_w AND L_e: METHOD B, 12.

	32°	36°	40°	44°	48°
Warm limen					
Kraeuter	.079°	.268°	.327°	.200°	.358°
m.d.	.015	.082	.083	.050	.062
Lively	.062°	.679°	.974°	.508°	.637°
m.d.	.016	.091	.149	.119	.134
Cold limen					
Kraeuter	—.036°	—.225°	—.121°	—.333°	—.113°
m.d.	.018	.082	.094	.055	.069
Lively	—.178°	—.163°	+.033°	—.959°	—.528°
m.d.	.018	.115	.131	.160	.168

iv. Method B, 12.

This method is analogous to method A, 2, one hand being exposed to S and the other to V, simultaneously; adaptation to 32° in all cases. Two observers (K and L) covered the range from 32° to 48°. The results are quite similar to those in B,11 and can be briefly summarized: (1) With both K and L, as in

[10] Reducing the exposure-time would help to increase the limen.

Notes to Table XVII and Figure 7.

The figures given are, in each case, the means of from 14 to 32 separate determinations; see Appendix, p. 131. As in A, 11, the mean deviation is shown instead of the p.e. The values at 48°, where pain is very obtrusive, are shown but not used in the adjustment equations.

THE ADJUSTMENT EQUATIONS.

	a	*b*	*b*	*Weighted deviations* 32°	36°	40°	44°
Warm limens							
K:	.0789°	.0691°	—.0049°	.000	.001	—.001	.001
L:	.0613°	.2280°	—.0157°	—.003	.005	—.005	.002
Cold limens							
K:	.0373°	.0287°	+.0004°	.004	—.013	.013	—.004
L:	—.1753°	—.0153°	—.0006°	—.010	.008	.024	—.024
L_w-L_c							
K:	.1162°	.0978°	—.0053°	.004	—.012	.012	—.003
L:	.2366°	.2433°	—.0151°	—.013	.013	.019	—.022

Quadratic curves ($y = a + bx + cx^2$; $x = 0$ at 32°) are laid through the observed values and fit the warm limens quite well, especially in the case of K; the cold limens are so erratic that the deviations run high. In forming the normal equations, by the method of least squares, each figure is weighted inversely to its m.d.^{2}; and in finding the weighted deviations of the adjusted from the observed values, this process is reversed so that each deviation is reduced to unit weight.

B, 11, the interval L_w-L_c drops from 44° to 48°; curiously enough, L_w increases but the decrease in L_c more than compensates. Our previous analysis is hereby confirmed, that in the case of these two observers the emergence of pain provides criteria of greater accuracy than the thermal ones which are more or less displaced. (2) The course of the successive values for L_w-L_c from 32° to 44° is, in both cases, so clearly curvilinear that only the quadratic adjustment is given. This confirms our finding in B, 11 that the rate of increase in the limens is a function of adaptation; cf. p. 77 and p. 101. (3) We can here separate L_w from L_c, as we could not in B, 11; and we now find that the increase in the cold limens is much more closely rectilinear than in the warm ones; this appears from the much smaller values of "c" in the adjustment equations (.0004 and .0006 as compared with .0049 and .0157). If this proves to be uniformly true, it provides an inference of no little consequence for a theory of the thermal senses: that the cold organs are less subject to adaptation than are the warm ones; cf. p. 102.

We turn now to the constant errors in the three forms of the method of limits: A, 11, B, 11, and B, 12. In addition to the

errors of time, space and direction of movement (p. 63), the method of limits tends to induce an error of "expectation"; this is likely to appear also in the constant methods (cf. p. 62), but is more direct and obvious here. In a down-series, for example, observer knows that he is approaching equality, and may therefore change his judgment from "warmer" to "equal" sooner than he would have done on the basis of the sense-impression alone. This error equals ½ (Down-limen minus Up-limen); it is positive, therefore, whenever the change (from warmer to equal, or equal to cooler) in a descending series appears at a higher point than the corresponding change (equal to warmer, or cooler to equal) in an up-series.

TABLE XVIII.

	Space-and Movement-Errors					*Errors of Expectation*				
	32°	36°	40°	44°	48°	32°	36°	40°	44°	48°
1. Combined					*Method A*, 11.					
G:	.000	—.006	+.026	+.061		—.014	—.042	—.090	—.110	
M:	+.004	+.016	+.024	+.048		—.046	—.035	—.052	—.101	
					Method B, 11.					
G:	.000	—.034	—.054	.000	+.142	—.014	+.012	—.062	—.050	—.059
M:	+.004	+.031	+.103	+.050	+.066	—.046	—.006	—.016	—.062	—.072
K:	+.005	—.039	—.041	+.002	+.181	+.047	+.104	+.051	+.089	—.008
L:	—.008	—.012	+.041	—.030	+.112	+.010	+.032	+.008	+.049	—.096
2. Space alone.					*Method B*, 12.					
K:	+.022	—.204	—.352	—.081	—.037	+.077	+.112	+.070	+.130	+.156
L:	+.007	+.362	+.514	+.460	+.366	+.033	+.281	+.464	+.429	+.388

The error of expectation is shown for each observer by each method. No time-error is shown, for the reason that none exists in B, 12, while in A, 11, and B, 11, where only one time-order (S to V) was used, it is indeterminable. For this reason also we cannot separate the errors of space and movement; the figures given represent the combined influence of tendencies due to the spatial relations of S and V as well as to the direction of movement in passing from first to second stimulus. Since the error for L_w and L_c, in each case, is about the same and no reason appeared for printing them both, only the mean of the two for each temperature is shown in Table XVIII.

In the combined space-movement errors, the following appear to be worth noting:

(1) Of the four O's, only one is consistent throughout; M uniformly overestimates the stimulus on the left, in A, 11 and B, 11; the tendency also increases above 32°. The fact can only be explained, in our opinion, as was the same situation in

A, 1: that M's left hand is more sensitive than the right, and the hands are so immersed that the response is largely determined by impressions from the left hand when V is to the left and from the right when to the right (cf. p. 65).

(2) In B, 12, L shows a positive error throughout while K's is predominantly negative; the tendency again increases above 32°. The most obvious causal factor here is the unlike discriminative indices of the two hands. L's positive error shows that her left hand was more sensitive to warmth than was her right—a fact confirmed by her own testimony: "my left hand seems more delicate, and responsive to thermal impressions than the right one." K's right hand, on the contrary, was obviously more sensitive than his left.

(3) When paradoxical cold (44°) and pain (48°) clearly emerge in B, 12, the error of both K and L falls off, whereas we should expect it to keep on increasing as it does from 32° to 40°. This seems to mean that greater sensitivity of one hand to warm impressions does not imply equally greater responsiveness to cold or painful ones.

The errors of expectation reveal a striking difference: those of L and K are, in nine of ten cases each, positive; those of G and M are, with but one exception for G, negative. K and L, therefore, show the influence of expectation, anticipation; while G and M have a conservative or perseverative tendency.[11] Inasmuch as up and down series followed each other in regular order, each of these tendencies moved alternately in opposed directions; we may therefore assume that the accuracy of discrimination was but little affected by them.

G and M were, in our opinion, the most consistent subjects of the group; and their conservative tendency may well be taken as a sign of careful observation.

2. *Qualitative Description and Comment*

Whenever the observer's comments could be fitted into the

[11] G is the only observer who commented on this point; he reported: (Practise series, 32°) "When differences get small, expects 'equal' to appear; when the stimuli are equal, does not expect a change." "If in any doubt, repeats previous judgment instead of changing."
(32°) "The first 'equal' judgments in several series came as a kind of surprise, that is, sooner than expected."
(A, 11; 36°) "Knowledge of the direction in which the variable is moving influences him negatively; that is, in doubtful cases, tends to repeat previous judgment if not entirely sure."
These comments clearly reveal G's conservative practise and thus confirm our inference from the negative figures above.

objective data which they confirm or explain, this has already been done; but we still have a few matters of general interest which do not belong with any particular group of data or which should be treated together; these will be presented here.

a. The Process of Comparison.

i. The Instruction or "Determination" of the Observer.

Being an integral part of the whole experimental setting, this topic might well have been proposed in the chapter on Procedure (Chapter IV); it is so closely related, however, to the observers' comments on criteria, sources of error, movement of attention and other "subjective" elements in the discriminative process, that we have deferred its treatment until the present. We need not stay to urge the importance of this matter; in the complex out of which a given response eventuates, such as a thermal judgment, the intraorganic factor of set or determination is a no less crucial determinant than are external conditions.

In our study the problem of instruction really amounts to this: the observer must be rigorously trained to compare stimuli from temperature quality *alone* for the whole period of the experiment, in order that our data may consist of nothing but purely thermal discriminations. To this end, by means of preliminary series, each observer was given oft-repeated instruction and training in three modes of thermal experience: in the elemental phenomenon of Warm and Cold, what these qualities are like; in the phenomenon of transition or change as Warm follows Cold or Cold replaces Warm; and finally in the experience of passing from Warm to Warmer and from Cool to Cooler. One who is unversed in this kind of investigation might well believe these experiences too obvious to need special study or clarification; in careful work, however, we soon are faced with statements like the following by G, upon beginning at a new level: "in the first series, I was tempted to say 'equal' early, because I did not know what 'equal' felt like at this temperature"; later in the hour " 'equal' was more easily recognized." In all these practise series we used the method of limits, where the direction of change is known to O, so that after repeated experience of the transitions from warmer to equal to cooler and reverse, he might get a firm and clear apprehension of just what those terms mean. Little

can be done in the way of verbal instruction to the observer; all we can do is to put him into a situation where warm and cool impressions occur in close juxtaposition and then let him develop and fix in mind the criteria to which in future those terms will be applied. The gross discriminations of warm and cold in every-day life must be clarified and dissociated from all irrelevant criteria.[12]

We cannot refrain from devoting a paragraph to this point, as a matter of psychophysical (and general psychological) methodology. Some investigators seem to find much profit in recording, with meticulous fidelity, an O's verbal ("introspective") comments on his procedure and state of mind in the act of comparing stimuli. But surely our purpose, in a study of this kind, is not to learn what an untrained O does or how he "feels" when presented with two thermal stimuli; what we need is to have O do precisely what we want him to do; and for this purpose, even the best O must be trained. We believe that much psychophysical and other psychological experimentation has grossly infringed this elemental principle of adequate technique. It seems to be a general and unquestioned presumption that, if preliminary work be maintained until "practise-effects" have ceased, the observer is then adequately primed for his task. This follows not at all: *uninstructed practise may serve only to fix and intensify wrong procedure.* In our case, for example, an observer by dint of long "practise" may simply refine his use of non-thermal criteria, and thereafter keep on discriminating stimuli in terms of pain and pressure qualities instead of by warm and cold. The need of proper training shows with special clarity in these total-impressions where thermal qualities are so neatly immixed with other forms of somesthetic sensibility and which seem also peculiarly liable to sudden and curious changes and irruptions of one kind or other, which disturb the process of comparison. While no pretense is made that our observers (even E-O, whose training was by far the best) gave nothing but clean thermal judgments; that is negated by their own testimony more than once; yet this very fact shows the need of proper training.

This preliminary initiation was longest and most intensive for E-O, whose total practise-period covered some 110 hours, by various forms of the method of limits and constant stimuli. So long a time was not needed for this purpose alone, to be

[12] Cf. articles VII and X in the writer's Studies (*7b*).

sure, but inasmuch as the objective results show that discrimination continued to improve throughout this period, no data from it are used except in one case (p. 74). The writer was enabled thereby to scrutinize every phase of the observer's problem for sources of error and possible difficulties; to get firm hold of the thermal criteria and keep them clean of spurious ones; and on the whole to gain a very considerable degree of skill in temperature discrimination. Observer Geissler, being experienced in psychophysical work, soon got well hold of the true and specious criteria and the several sources of error, and commented on them freely; and while his introductory period was very much shorter than E-O's (about 7 hours), he seemed to be well apprized of the crucial points. The situation with the remaining O's must be induced largely from their data. Mulvey was objectively an ideal observer, painstaking, consistent and confident in her work, though without technical training; her close agreement with G seems to assure the quality of her results. The work of Kraeuter is what may be expected from any intelligent, psychologically unsophisticated individual who has no great interest in either problem or method. Lively, though careful, was pretty erratic; she never seemed to be quite sure of her criteria (cf. p. 78) and also had a penchant for "trying out" various methods extemporaneously to see what would happen.[13] She thus picked up a good many suggestive and even valuable comments; she is obviously better fitted for qualitative description than quantitative determinations; but her failure to maintain a uniform set or procedure both helps in explaining the unreliability of her data (her p.e.'s run higher than those of any other observer; see tables for B, 11 and B, 12 on pp. 79 and 81) and justifies us in giving less weight to her conclusions.

In brief, the writer believes we may look upon our data as *genuine thermal limens* won by a procedure carefully imposed and consistently held; by E-O perhaps best, by G and M in high degree, by K in ordinary measure and by L perhaps least.

Our formal instructions for all observers will now be presented. During the inaugural stage of our investigation, with

[13] Typical examples: "At first today, I gave more attention to the stinging quality, hence fewer 'equals' in the early series; later, I waited for the temperature change, then 'equals' became more numerous." "Today and yesterday have been using 'auto-suggestion' to see if actively anticipating a change in judgment (say, from warmer to equal) would affect the actual discrimination."

subjects G, M, K, L, by method of limits, which anteceded E-O's observations with constant stimuli, the following instructions and memoranda were formulated with much care and precision; they were then used, with but minor verbal changes to fit them for various methods, throughout the whole course of our study.

Specific instructions:

"You have hitherto received careful instruction in three modes of thermal experience: in the elemental qualities, warm and cold; in the succession of these qualities, the transition from either one to the other; and, thirdly, in the transition from warm to warmer and from cool to cooler. This preliminary training was designed to prepare you for the service you now are asked to give.

"In each trial you will expose either the same receptive surface in turn, or different receptive areas at the same time, to the stimuli S and J; you will in consequence experience two thermal sense-impressions, whether successively from the same area or simultaneously from separate ones. On the basis of the purely thermal impressions alone, you will, in each case, announce whether the second stimulus (or, J) is warmer or cooler, that is, of higher or lower temperature, than the first (or, S). When there is no perceptible difference in the purely thermal quality of the two total-impressions, you may call them 'equal' or 'doubtful.'

"You will, of course, seek to observe, not with uneasy scrupulosity, but with assured care and attention. At the conclusion of each day's work, you will be asked to report briefly on any factors you may have noticed, whatever their nature or degree, which would tend to shed light upon (to confirm or explain) your results."

General Memoranda:

"The preliminary training-series have made you familiar with the general method of the experiment, and have also enabled you to appreciate certain factors which are briefly noted below. These factors are here recalled and emphasized, not at all with the implication that you have been ignorant or neglectful of them, but only to bring simply and sharply to mind what you have learned, in a general way, for yourself.

"Since the experiment consists of successive comparisons of two temperatures as the difference between them is changing, the chief obligation of the observer is to make the comparisons with care and independently; that is, each one for itself. In this method, as you know, each judgment is part of a series which moves in a definite direction, from Warm to Cool or from Cool to Warm. Now this knowledge that the series is directed up or down may, if not provided for, have an influence upon one's judgments at the turning-points. For example, after giving a number of Warmers or Coolers, the observer may be led to change, as the differences grow smaller, because he knows that the change is bound to come and feels that it is about due; or if one has given three or four equal judgments in several series he may fall into the way of giving that number of Equals every time. Suggestive influences of this kind are often so subtle that the observer is quite unaware of their presence or operation; but if active, they always appear in the results. The *one* way to nullify them is to be sure that every judgment is determined by the actual sense-impression alone, with no reference to the rest of the series.

"In this connection it may not be amiss to point out that, although you know the direction of each series, there are two important facts about it which you are not permitted to learn: 1) the exact point where

a series begins; and 2) the rate of rise or fall in the temperature. Except by chance, no two series, up or down, begin at the same level; hence the number of steps will be affected. Secondly, the rate of rise or fall in the temperature is not uniform for different series nor even within the same series; it depends upon the amount of current admitted to the heater-lamp, the amount of ice in the jar, the temperature of the room, and other variable factors. All this goes to confirm, that each series and each decision must be taken for itself; what holds of one does not hold for another.

"Hitherto you have been, in minor details, left free to choose and develop your own methods of comparison; in the future, however, these must not be varied. For example, your position by the table, whether sitting or standing, the way you hold your hands and fingers during immergence, the depth to which you immerse them in the water—any factors of this kind should be kept as nearly constant as possible. They may seem small and even trivial to you, but a change in these matters will be sure to introduce disturbing variations into your results."

ii. True and Specious Criteria

We now consider the two kinds of criteria: the specious or non-thermal ones, whose character must be clearly defined in order that their presence may not help to determine and thereby vitiate the response; and the true thermal ones, their nature, locus and temporal course.

(1) Specious criteria[14] were reported as follows:

(i) Tactual complexes (numb, smooth, glow);

(ii) Pain complexes (tingle, sting, prick, burn).

[14] In implying that these qualities are illegitimate criteria, we by no means prejudge the question: do thermal and other somesthetic qualities have an underlying kinship? The relation of cold to pain and of warm to pressure is far from being adequately explored and may well prove to be more intimate than we now know. At extreme temperatures we encounter pains and pressures which are hard to distinguish from colds and warms; even the most careful observer is not always sure where the true criterion ceases and the spurious one begins. By the present distinction, therefore, we only mean that certain qualities, which can ordinarily be differentiated from warm and cold, appear in the total experience and may serve to confuse discrimination.

E-O reported as follows: (20°; method A, 2) "Upon first adapting to this temperature today, a burning, stinging quality appeared, which persisted for a half-hour or so; thereafter the temperature was no longer painful, but hands remained slightly numb." (20°; method A, 2) "Judgments today apparently influenced in some cases by tactual factors; that is, when one hand feels less numb there is some tendency to call it warmer." (20°; method B, 2) "Left hand often stings today; this tends to confuse temperature-discrimination." (40°; method A, 1) "Slight burning, stinging effect, not at all intense, seems to appear in jar which is called warmer." (40°; method A, 2) "Left hand, especially the large finger, gave a stinging quality during the last three series today; this may have led me to estimate the left stimulus warmer." (44°; method A, 1) "Two jars today gave a stinging effect [cause undetermined; not due to temperature] so intensive as to disturb discrimination and make them seem decidedly 'warmer' on the basis of these spurious criteria. The temperatures were measured at the close of the day and an astonishing difference in secondary criteria appeared between jars whose actual temperature was almost the same." (44°;

The cited statements reveal what kind of non-thermal qualities enter into the total perceptive complex; and confining our interpretation to E-O and G, we find:

(1) In only a few cases do the specious criteria become so conspicious or disturbing to trained observers as to be worthy of special comment; they seem to crop out suddenly and after a few hours again recede into the background.

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method A, 2) "A kind of tingling quality most of the period today, but in some jars more than others; it becomes less noticeable in time, but persists with considerable clearness and force throughout. Seems due not merely to higher temperature of water, but to other undetermined factors as well. Not believed to have affected discrimination particularly."

G reported as follows: (32°; practise series) "It seemed to go easier today; criteria much more certain and reliable. No disturbance by secondary [specious] criteria." (32°; method A, 11) "Did not feel so sure about my judgments this hour, especially at the transition-points (slight headache and poor attention). Was not sure sometimes whether apparent difference due to secondary or principal [specious or true] criteria." (36°; method B, 11) "Upon beginning work at this temperature, was unfamiliar with secondary criteria; as soon as these were known and recognized, I had no more difficulty. When V was warmer than S, it seemed to have a stinging quality in addition to temperature; the gradual disappearance of this tingling gave warning of the approach to equality. When V was cooler, it seemed to have a more smooth, velvety quality, whose disappearance indicated equality. These were the important secondary criteria; by end of hour, however, they had practically disappeared." (44°; method B, 11) "Temperature-impression rises slowly to full intensity: first, a slight prick (shock-like), then comfortable warmth, changing into a prickly quality, particularly on back of fingers and on knuckles. Prickling especially prominent in V when it is warmer than S. 'Equal' judgments are based partly on absence of prickly quality and greater evenness of temperature all round the fingers. Judgments of warmer, cooler, equal during this hour were really abstractions from secondary criteria, that is, slight pressures and pains, which are much like temperature. [It should be noted that this was his *first* hour at this level, 44°]. (44°; method A, 11) "Judgments based entirely on temperature today; no pain present." (44°; method B, 11) "Transition from 'equal' to 'warmer' during this hour sometimes based on a qualitative difference, a kind of diffused 'glow', which is perhaps the beginning of pain but is not distinctly painful." (36°; method A, 11) "At this level the impressions are *pure* temperatures; no pain component and tactual qualities are insignificant.'

L reported as follows: (32°; method B, 11) "Warm has a close, snug tactual character, while cold has less of the pressure-quality. This tactual difference aids in distinguishing temperatures, especially from 'warmer' to 'equal'." (36°; method B, 11) "V gives a stinging quality when it is getting warmer than S. As the difference becomes small, the stinging changes more than does the temperature. At first today, I gave more attention to the stinging quality, hence fewer 'equals' in the early series; later, I waited for the temperature change, then 'equals' became more numerous." (36°; method B, 11) "Today I used the stinging quality as a criterion in the left [space-order] series, but this factor was less prominent, or attended to, in the right series. Stinging appears upon passing from the adaptation-jar (32°) to S (36°); then if V is warmer than S, it grows more intense; if V is cooler, it disappears or is less noticeable. Yesterday and during the left series today, this sting-

(2) Even in the cases reported, we may well ask whether non-thermal qualities disturb the objective response as much as O himself is inclined to believe. For example, at 44°, method B, 11, G once reports that his judgments were "abstractions from secondary criteria"; but the actual limen at 44° fits in well with the whole curve and departs widely from that at 48° where secondary criteria really begin to dominate the complex (see Table XVI). Being set to look out for them, O may well be less affected by spurious criteria than he himself thinks.

At 48° we find pain so obtrusive and insistent that all other qualities recede and even the most careful observer can hardly give a purely thermal discrimination; all values at this level are therefore kept apart from the others.

(2) True Thermal Criteria

(i) Their Nature.

Of these we find but two: a change in the *quality* or *intensity* of a thermal impression as we pass from one stimulus to another (or a corresponding disparity in two coexisting impressions from separate sensory surfaces). Thus Warmer is announced either (1) when J gives a warm while S gives a cool impression; or (2) in case both impressions are warm, when that of J is more intensive than the other.

The above may seem too obvious for mention, as purely formal statements often do; but the meaning of these words,

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ing was the main criterion used for discrimination." (40°; method B, 12) "Today I judged by temperature rather than by stinging."

K and M made no specific comments on this point.

At 48°, which stands alone both objectively and subjectively, the following comments were received:

E-O: (Method B, 2) "Both tactual and pain complexes (drawn contracted skin and stinging effect) often help to determine the judgment 'warmer' here."

G: (Method B, 11) "In passing from 'warmer' to 'equal', I sometimes depended on intensity of pain as well as of temperature; that is, when not sure from temperature alone, I relied on pain criteria." "Relies on stinging quality in doubtful cases." "In first series today, judged on basis of pain quality."

L: (Method B, 12) "Stinging is a factor in judging here, but do not know relative importance as compared with true temperature quality." "Proceeded somewhat differently today; tried to disregard the stinging quality and judge *only* from temperature by not keeping hands immerged long enough for the stinging to develop." (Method B, 11) "Easy to tell the difference here; relies upon pain quality rather often."

K and M both reported that pain was a factor in discrimination at this level.

as a matter of fact, can only be gained or imparted by a special course of training such as ours, wherein O repeatedly compares two thermal impressions while the difference between their stimuli is changing in a direction or manner known to him (cf. p. 85 above). He thereby learns the necessary and sufficient criteria for (1) a change from Warm to Cool, and (2) a change of intensity, that is, from Warm to Warmer and Cool to Cooler.[15]

(ii) Locus.

At first observers tend to report, thermal impressions are referred to the whole receptive surface uniformly; upon closer analysis some differentiation begins to appear, as follows:

Warm noted specially on back of fingers (G, L) and around joints (E-O, G); Cold in creases of skin (L, E-O) and fingertips (E-O); both near hand, where water and air meet (E-O, G, L) and in left hand more than right (E-O, L). No observer seems to have an invariable locus for either group of impressions.

(iii) Temporal Course.

The thermal perception of S and of J each continues throughout a two-second interval from the first to the final impression; does the intensity of the two complexes run a uniform or variant course in this period?

The intensive relation of the two perceptions belongs in theory under one of four heads, all of which are found in practise:

(a) parallelism.
(b) divergence.
(c) convergence.
(d) some combination of these three.

(a) Sometimes, with adaptation to S, when the air is of such temperature that no appreciable time-error is induced from passing the receptive surface through the air, the initial impression does not perceptibly change throughout the interval: if the two appear equal upon immersion, they remain so. Both may warm up or cool down but they do so at the same rate.

[15] The writer has treated elsewhere the general (and basal) question: "What do we mean by an *intensive* and by a *qualitative* change in perception?" Cf. article VII (7*b*).

(b) But the relative intensity of the two complexes rarely continues wholly without change through the period of exposure; as a rule their disparity increases and the two seem to diverge. The impression may begin with a kind of total effect, devoid of differential characters; a variety of specious criteria may come in to distort and obscure the picture; but these tend to fade rapidly away and leave the thermal perception standing forth in more or less clear relief.

Thus, G reports: (44°; method A, 11) "At high temperatures, the impression rises but slowly to full intensity." E-O: (32°; method B, 2) "Sometimes response determined by fact that left hand grows warmer while no apparent change occurs in the right." "Sometimes the judgment seems to spring naturally from one hand, sometimes from the other—apparently the one which changes most from adaptation-impression." "At first little difference can sometimes be detected but it later becomes distinct."

(c) In a few cases, when the stimuli differ but little and lie near skin-temperature (32°), where adaptation is most rapid and complete, the receptive area seems to adapt so readily that two impressions, which differ perceptibly upon immersion, become indistinguishable by the close of the two-second interval; in this case the disparity decreases and the two perceptions converge. This was repeatedly observed by E-O and also mentioned by K (36°; method B, 11).

(d) Finally, certain combinations of the preceding (say, both convergence and divergence) may appear within a single interval of exposure. Thus E-O reports: (20°; method B, 2) "In some cases the right jar feels cooler upon immersion but at the close of the two-second period the left jar feels cooler. These cases are reported 'doubtful' unless one of the 'coolers' seems more intense than the other." These reversals, however, do not appear frequently.

In view of these possibilities within the exposure-period, it is obviously desirable that the response be delayed until the moment of withdrawal; in order that all the "evidence" may be in before any judgment is given and that all judgments may be uniform in this regard. There is some tendency for a novice to depend upon the first impression; thus, during the early stages of practise, G reported that he "judges from the *first* impression" and M that she relied upon the impression "when hand enters water or just a 'moment' later." But the tendency is soon overcome; their later reports and the writer's own scrutiny of their procedure concur, that, apart from aberra-

tions now and then, the final formulation of their judgments was deferred until the moment of withdrawal. From the start this was E-O's method, consciously adopted and carefully observed. The need for it is clear from his own comments:

(32°; method A, 3) "Response is sometimes determined by slight change appearing just at end of two-second interval." "Sensory impression does not change consistently during the exposure-period; sometimes it gets warmer, in other cases initial warm is followed by indifferent or cooler." (36°; method A, 1) "Upon immersing hands, water regularly seems warm and quickly cools down [owing to time-error]. Discrimination is then based upon this criterion: whether the impression by the end of two seconds, still seems warmer than did the preceding one after the same interval; the rate of change (cooling) may also be a clue."

In our study, therefore, the judgment "warmer" means on the whole one of two things, insofar as the temporal course of the perceptions is concerned: (1) that J appeared warmer than S in some portion of the exposure-period, even though "doubtful" before or after; (2) that, in case J appeared (rarely) both warmer and cooler in succession, its "warmer" seemed more intensive than that of S. The meaning of cooler is, *mutatis mutandis,* the same. In theory, it is not a matter, as the reader will note, of preferring the final impression to the first one; but rather of deferring the response until the whole temporal course of each perception is known. The final impression, to be sure, coming as it does at the time of maximal attentive clearness and after the sensory Anstieg has reached its acme, will to that extent dominate the whole; but in the case of two perceptions which "converge" during the exposure-period, the difference of the earlier impression was reported rather than the "doubtful" of the final one.

iii. Movement of Attention.

G, as a rule, reports uniform and fairly diffuse attention, not being specially directed to particular impressions or individual portions of the hand.

With E-O were three pretty well defined forms:

(a) Passive (perhaps most used in simultaneous methods).

Some fluctuation of attention from one hand to other, but no active direction or regular movement to and fro. Impressions taken passively, until one hand comes out as cooler or warmer, whereupon eyes tend to move in its direction and visual imagery of that hand becomes more or less clear. Slight

throat or head movements often accentuate the metronome beats.

(b) Active (simultaneous methods).

Active and regular movement of attention from one hand to other during exposure-period, thus: S-J-S-J or J-S-J-S; in either case we pass once from J to S and once from S to J, with a final "swing-back" to the other hand at the moment of withdrawal. We thus have each impression at maximal strength, after the same period (2 seconds) of stimulation and while it is still clearly present in the memory after-image. In this movement of attention, each hand was in turn visualized more or less clearly and the eyes shifted in its direction; simple kinesthetic patterns, in time with the metronome (head and throat movements), were often used to accentuate the impression from a given hand, to focalize it for a moment, and thus aid clear apprehension and discrimination.

(c) Active (successive methods).

Here attention fell into three accents, in time with the strokes of the metronome (upon immersion, after one second, and at the time of withdrawal), in a kind of attentive and intensive crescendo, the last and sometimes the second being fully clear and maximally intense, the first being preparatory. This was repeated in the second jar and any difference in the whole thermal situation touched off the response. Here again kinesthesis of head and throat seemed to be of aid in bringing an impression to clear relief.

The above schemata were developed early in E-O's work as an observer; they were not followed with pedantic fidelity but served as a kind of pattern and guide.

b. Description of Thermal Qualities

The main epithet applied to warm is "pressury"; to cold is "light" (tactually), smooth, less pressury than warm. Consider the following reports:

L: (32°) Warm has a "close, snug quality"; (36°) "Warm water seems to exert greater pressure against the skin." G: (36°) "When V was warmer than S, it seemed to have a stinging quality in addition to temperature." (44°) Judgment "warmer" sometimes based on a "kind of diffused glow—perhaps beginning of pain but not distinctly painful" [paradoxical cold is undoubtedly a part of this complex].

L: (32°) Cold has "less pressure"; (36°) a kind of "lightness" (tactual). G: (36°) "Has velvety, smooth tactual quality." E-O: (32°)

Cool has kind of "tactual lightness." (24°) "In left hand, cool is more diffuse, slightly painful; in right hand is sharply localized, not numbing or deadening." (20°) "Cool to left hand is diffuse, numbing; to right is stinging, penetrating, localized in spots."

The pressury character of Warm and the sharper, even painful, nature of Cold thus seems to emerge from our reports. The writer has no special comment to add, except to record his opinion that we shall yet find an underlying unity of warm with pressure and of cold with pain more intimate than we perhaps suspect.

c. The "Equal" and "Doubtful" Judgments

Throughout the method of limits, "equal" was used as one of the three categories of response open to the observer (Warmer, Equal, Cooler), whereas in our preliminary work on constant stimuli, "doubtful" was also applied. We thus got material for answering the question:

Which type of response is methodologically better for the intermediate category—"equal" or "doubtful"?

The answer will depend upon a second question: is either "equal" or "doubtful" an *indispensable* category, without which O cannot always do justice to the psychological situation in which he is placed? By adopting the Urban method in its standardized form, we have already been committed to the use of *some* intermediate category in addition to the two extremes (Warmer and Cooler); having done so, we must now be sure that our intermediate form of response is adequate to O's every need.

As for Equal, L and G, who alone comment on the matter, agree that not all "equals" are alike. L once reports, to be sure, that she is "not doubtful about 'equal' judgments; they are as certain as the others;" she finds, however, that two impressions may differ in thermal quality and yet, owing to imperfect mastery of the criteria, she cannot be sure which is warmer. These do not really fit any of the three categories, but she calls them "equal"; whence it follows that, while she is certain of some "equals", she is not certain about all. Her repeated request, likewise, at the close of a series that, say, "the first two 'equals' be changed to 'warmer'," shows clearly that two impressions which at the time appeared equal did so no longer after other "equals" had been experienced. Early in practise, G reports "no difference between 'equals'"; but with closer

analysis, he finds: "Some 'equal' judgments are not very good ones. One 'equal' may seem better than the others—the recognition of equality is more certain. Some are not different enough to be called different but are not so good 'equals' as others." Later he says: (44°) "Equals more uniform today; no small differences between them."

From the above it is clear that, if "equal" be admitted as one category, "doubtful" must also be used to receive the truly uncertain cases; for mathematical analysis, however, the two can hardly be held separate and are in fact usually combined; hence why use "equal" at all? The contention that "equal" may be just as certain as "warmer" or "cooler" is irrelevant; the question for O is simply: of these two sensory impressions, is the one from J warmer or cooler than the other? When the two are subjectively equal, then "doubtful" is the true and only answer to this question.

Apart from practise series, therefore, the writer in his own observations with constant stimuli, used nothing but "doubtful" throughout; including thereunder the cases which reveal no apparent difference as well as those where the direction of difference is uncertain.[16]

d. Absolute Impression and Tendencies of Judgment

The doctrine of absolute impression was introduced by Martin and Müller (*15*, p. 45ff.) for the purpose of explaining two experimental results: the "generelle Urteilstendenz" and the "typische Urteilstendenz."

Müller defines the "general tendency of judgment" as follows: "es besteht im allgemeinen eine Tendenz, bei gleicher wirksamer Differenz mehr richtige Fälle zu ergeben, wenn das Vergleichsgewicht das zuzweit gehobene Gewicht ist, als dann, wenn dasselbe an erster Stelle kommt" (*18*, 114). The phrase "equal effective difference" (gleiche wirksame Differenz) is here of special consequence and may be defined as follows. When a given time-order effects an increase in the number of correct responses, we may, as Fechner suggested (*11*, I, 115), regard it as increasing the objective difference of stimulation (D) by a given amount, P; the effect of space-order, in like manner, is Q; the effective difference therefore equals $(\pm D \pm P \pm Q)$ as opposed to the mere stimulus-difference

[16] This question is more adequately treated in article VIII of the writer's Studies (*7b*).

±D. Now consider the four time-space combinations when J > S (+D) and J < S (—D):

	I	*II*
1.	+D+P+Q	—D+P+Q
2.	+D—P+Q	—D—P+Q
3.	+D+P—Q	—D+P—Q
4.	+D—P—Q	—D—P—Q

We find that each of the four in one group is a direct negative of one in the other group; thus, (I, 1) = —(II, 4). In I, 1, J is larger than S by (D+P+Q), to the left and second; in II, 4, S is larger than J by (D+P+Q), to the left and second; we thus have the same "effective difference" and should get the same percentage of right judgments (a slight difference from the fact that the first stimulus is a little smaller in II, 4 need not concern us here). On the contrary, Müller found, in the case of all his observers (13) and with a variety of experimental procedures, that these pairs by no means give equal numbers of right judgments: time-order S-J on the whole returns more "rights" than does J-S.

A "typical tendency of judgment" appears when the sum of the right responses for all the combinations of I (+D) is not equal to that in the case of II (—D). When the latter (—D) has more "rights", the type is positive; when fewer, the type is negative.

Müller sets out to explain these tendencies with his doctrine of absolute impression: that a given judgment (heavier, lighter) may issue from an actual comparison of S and J, but may also eventuate merely because one of them seems "absolutely" heavy or light without regard to the other ("how heavy this bag is!").[17]

Two questions now engage us: do these tendencies appear in our own judgments? and do they spring from inadequate

[17] Müller's analysis and doctrine are affected with grave inadequacies which are examined with some care in article II (*7b*). For example, none of the four equations (I, 1 = — II, 4, and so on) is correct; we have already shown (p. 63) the existence and operation of a third time-space factor (M) which is on a par with P and Q; we cannot pass it by as non-existent or negligible. The true combination for I, 1 is +D+P+Q+M and for II, 4 is —D—P—Q+M; if we now try the equation I, 1 = — II, 4 we find it does not hold; the two members differ by 2M. This is true of the four pairs which Müller equates; but the matter shall not delay us now, since the above will meet our present need.

technique; that is, are they eliminable with appropriate modification of procedure?

First, do these two tendencies leave any trace in our own data? The evidence is of two forms: objective and subjective. (1) *Objective.* As for the general tendency of judgment: we find the sum of the "right" percentages in 143 cases with a variety of stimulus-differences to be 95.45 in time-order S-J, and 97.98 in time-order J-S, a ratio of 100.0 to 102.7. The disparity of the two is noticeably small; whence we see that Müller's generalization fails to hold in our study; what tendency we do find is slightly opposed to his doctrine.[18]

As for the typical tendency of judgment: the sum of the "right" percentages for the 143 cases of group II (—D) is 96.38 as against 97.95 in group I (+D), a ratio of 100.0 to 101.6. Here again the difference looks too small to be of consequence. By Müller's usage, E-O turns out to be a trifle negative in type; or, to put it otherwise, he displays a slight preference for the judgment Warmer as against Colder.

(2) *Subjective.* The observer does not know, of course, when giving a judgment whether it is "right" or not; but we can often learn, from his verbal reports, whether his judgments are being influenced by "absolute impression." E-O reported as follows:

10. III: "Left hand seems affected for longer time than does right by application of cold; that is, effect of previous stimulation seems to hold over."
29. VII: "Left hand seems to get 'set' in a certain way, so as to give all warmer judgments for a time."
11. VIII: "Noticed tendency to give same judgment a number of times in succession (Cooler, Doubtful, Warmer), as by a kind of perseveration.'

These are related to the phenomenon of absolute impression, though it is not clear that any of them is a true example; they do show, however, that in E-O's opinion, his response was not always determined by the relation of a given pair (S and J)

[18] This may be explained as follows: In time-order S-J, E-O went from S, which served both for adaptation and for standard, to J and gave judgment, thereupon returning to S until the next trial; in order J-S, he went from S to J in preparation for the real comparison from J to S. In S-J, therefore, he made the transition but once before giving judgment, while in J-S he made it twice. The first transition from S-J was, to be sure, merely preliminary to the actual comparison of the two, but it might easily be of aid in judging and perhaps accounts for the small excess of rights in that time-order; but the extremely small disparity shows how well the two orders agree in accuracy.

alone, but was shaped or modified a few times by previous impressions which held over in some way.

The phenomenon of absolute impression has been found by many investigators (cf. Titchener, *24, d,* 304) ; indeed we may assume it to be almost inevitable, even with careful observers, when working by the customary procedure. If we have one S and seven J, S will be experienced seven times as often as any of the J, or in any event as often as all J combined. This preponderant frequency of S-impressions is virtually bound to give its criteria a stability and definition such as will tend to make any departure from S noticeable, even without an actual comparison with S; just as a confectioner's clerk can with the hand "weigh out" a pound of sugar with surprising accuracy. We may therefore accept Müller's doctrine that the comparison-stimulus is more likely to give an absolute impression of light or heavy than is S; that this impression will of necessity affect the judgment more when it appears with the second stimulus than with the first, is highly questionable; but the matter will not be taken up here (cf. art. II, *7b*).

We come now to our second question: can the effects of absolute impression be abolished by a change in the customary procedure? In large measure, certainly, if not wholly, by the method used in our study. The procedure was here adopted of necessity, because the apparatus was too bulky and expensive to be expanded beyond minimal requirements; but it also serves to make "absolute impression" of little or no effect, and thus merits consideration as a generalized psychophysical method. As previously elaborated (p. 45), six jars were used, each in turn serving as the standard; thus jar I was used as S for the first 20 trials, each of the remaining five being compared with it four times; then jar II was made S for the next twenty trials, all the others being compared with it four times each; and so on. By so doing we get fifteen stimulus-differences instead of only five, as by the usual method; and in addition we attain the important result that all the stimuli are experienced in the course of a period the same number of times. Jar I serves as standard twenty times and as J twenty times; so with II and the others. No stimulus therefore has the advantage of frequency over any other,[19] whence it is hard to see how any general trend could arise; and even if a ten-

[19] Unless, of course, more than one jar is given the same temperature.

dency to favor a given S appears, it should develop equally with all the S used in the course of a working-period; and hence would mutually cancel out in the final results. When we desire therefore to eliminate the "tendencies of judgment" instead of analyzing them, the above procedure may well be used.

3. *Comparative Conclusions*

The data above presented seem to justify the following conclusions: (a) Weber's Law (the existence of a constant ratio between increase of limen and increase of stimulus) is proved to be a *function of adaptation;* that is, under certain adaptive conditions, the constant ratio appears with notable clearness and consistence; in other adaptive conditions no approach to it can be discerned.

This proposition is readily demonstrable from our figures. If we fit the simple quadratic equation ($y = a + bx + cx^2$) to each of our liminal curves, the coefficient of x^2 ($= c$) will clearly show in every case how much a curve departs from rectilinearity. When c is zero, the curve is a straight line ($y = a + bx$), that is, the ratios are constant throughout and thereby conform to Weber's law; the magnitude of c, apart from sign, therefore, becomes a clear and convenient index of the amount by which the successive limens depart from a straight line (that is, from a constant ratio to the stimulus). Now consider these figures (the equation in every case applies to the interval between Warm and Cold limen):

Observer	*Method*	*Range*	*Equation*
E-O	A, 1	16°-24°	$y = .0811° - .0078°x + .0000°x^2$
		32°-44°	$y = .0791° + .0101°x - .0001°x^2$
	A, 2	16°-24°	$y = .1328° - .0097°x - .0008°x^2$
		32°-40°	$y = .1248° + .0052°x - .0002°x^2$
	B, 2	20°-32°	$y = .1331° - .2514°x - .0154°x^2$
		32°-44°	$y = .1339° + .1191°x - .0083°x^2$
G	A, 11	32°-44°	$y = .1506° + .0093°x + .0001°x^2$
M	A, 11	32°-44°	$y = .0853° + .0075°x + .0001°x^2$
K	B, 12	32°-44°	$y = .1162° + .0978°x - .0053°x^2$
L	B, 12	32°-44°	$y = .2366° + .2433°x - .0151°x^2$
G	B, 11	32°-44°	$y = .1510° + .0420°x - .0021°x^2$
M	B, 11	32°-44°	$y = .0845° + .0260°x - .0015°x^2$
K	B, 11	32°-44°	$y = .0783° + .0501°x - .0018°x^2$
L	B 11	32°-44°	$y = .1613° + .0993°x - .0061°x^2$

If we now put down merely the coefficient of x^2 (omitting sign) for the two groups A (adaptation to each J in turn) and B (adaptation to 32° throughout), and for the two methods (simultaneous and successive presentation), we get the following table:

		A	*B*
Successive		.0000°	.0021°
		.0001°	.0015°
		.0001°	.0018°
		.0001°	.0061°
	Mean	.00008°	.00288°
Simultaneous		.0008°	.0154°
		.0002°	.0083°
			.0053°
			.0151°
	Mean	.00050°	.01102°

These figures speak for themselves: the coefficient of x^2 in the B-equations runs from 20 to 35 times as large as in the A-equations. We need no statistical devices to show that a disparity so gross cannot be ascribed to chance; the two groups are of different order in magnitude. We have now established our first proposition: that Weber's law is somehow a matter of adaptation; that it appears only when certain adaptive conditions are fulfilled.[20]

(b) Cold seems to be less adaptable than warm, less amenable to a change of adaptive conditions.

Consider the evidence: the liminal curve for cold in group B above (adaptation to 32° throughout) departs much less from

[20] That this conclusion in no way issues from any peculiarity of condition or procedure in our study may be further shown by the following data from Abbott (1, 22); the entries are the DL for the several temperatures listed:

40°	$37\frac{1}{2}$	35	$32\frac{1}{2}$	30	$27\frac{1}{2}$	25	$22\frac{1}{2}$	20	$17\frac{1}{2}$°
1.7°	2.5	1.0	1.2	1.6	1.5	1.8	2.7	2.7	1.3°

The quadratic adjustment from $32\frac{1}{2}$° up, and down, gives these equations:

$$32\tfrac{1}{2}° - 40°: \quad y = 1.000° + .300°x - .024°x^2$$
$$32\tfrac{1}{2}° - 17\tfrac{1}{2}°: \quad y = 1.000° + .258°x - .014°x^2$$

These equations clearly reveal the same negative curvature as our own. Her limens run much higher than ours, being about twice as large; we should therefore expect the coefficients of x^2 to be larger than our own; the greater the absolute difference in successive limens, the greater will the curvature be.

the straight-line relation (Weber constant ratio) found in the A-equations than does the liminal curve for warm. Tabulated below is the coefficient of x^2 (which measures the degree of curvature, the amount by which a given line departs from rectilinearity) of each liminal equation for Warm and Cold separately in groups A and B.

	Equations for					
	A-Group			*B-Group*		
	W	*C*	*W-C*	*W*	*C*	*W-C*
	.0000°	.0000°	.0000°	.0138°	.0122°	.0016°
Succes-	.0001°	.0000°	.0001°	.0139°	.0071°	.0068°
sive	.0019°	.0020°	—.0001°	.0082°	.0067°	.0015°
	.0022°	.0022°	.0000°	.0130°	.0111°	.0019°
Mean			.0000°			.00295°
	.0005°	.0003°	.0002°	.0110°	.0044°	.0066°
Simul-	.0004°	.0006°	—.0002°	.0046°	.0037°	.0009°
taneous				.0049°	.0004°	.0045°
				.0157°	.0006°	.0151°
Mean			.0000°			.00678°

Under A we see that the coefficients of x^2 are in the mean precisely the same for both Warm and Cold equations; that shows that they both conform equally well to the linear increase demanded by Weber's law. In B, on the contrary, the x^2-coefficients for cold are uniformly and notably smaller than for warm. This means that the linear increase in both limens, which we find in A, is much less disturbed upon changing adaptation to 32°, in the case of cold than of warm. We conclude, therefore, that a change in the conditions of adaptation affects cold discrimination markedly less than warm.

(c) In this connection, we may note an apparent difference due to method. Other conditions equal, we seem to have a closer approximation to Weber's law with successive exposure of the same receptive area than with simultaneous exposure of distinct receptors (method of equivalents). Consider for the same observers, with conditions differing in adaptation alone:

	Simultaneous	*Successive*
A	.0008°	.0000°
	.0002°	.0001°
B	.0053°	.0018°
	.0151°	.0061°

The simultaneous coefficients run definitely higher than do the successive, which implies that discrimination by the latter procedure accords better with the linear increase of Weber's law than by the former. If this be a fact (the evidence is, of course, too meager for assurance), the writer has no explanation to propose.

CHAPTER VII

Theoretical Proposals

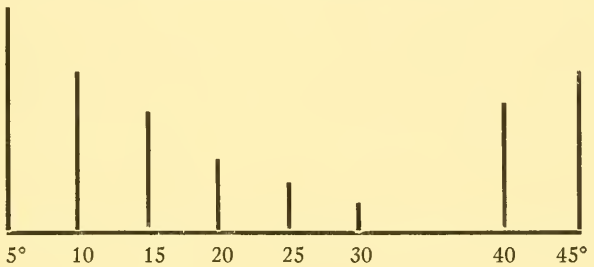
1. Explanation of Our Findings in Terms of Adaptation

Inasmuch as the two curves noted in (1) above—rectilinear and negative acceleration—can only be referred to unlike conditions of adaptation, we must seek to explain them in these terms.

Let us, to begin, adduce two facts from other sources. (a) Holm determined the adaptation-time at various temperatures from 5° to 45° (cf. p. 41 above). When a Thunberg Temperator was applied to various parts of the abdomen, the following times were needed, at the temperatures shown, for attaining a degree of indifference that was satisfactory to him (each value is the mean, in seconds, of five trials):

5°	10°	15°	20°	25°	30°	40°	45°
210	165	112	72	47	31	126	152

Graphically, we have the following curve:



A fairly uniform rise in adaptation-period from 30° (approximate skin-temperature) to either extreme, is obvious at once. The course of the ordinates from 15° to 45°, in particular, is notably similar to our own liminal curve (A, 1; p. 58) in three ways: in both, the minimum is near 30°, the curve rises more sharply above 30° than below, and the successive increments are roughly equal. We thus seem warranted in stating the following principles or rules as a preliminary approximation to the facts: The time required for adapting to

a given temperature-level (1) has a direct and almost linear relation to its departure from skin-temperature; (2) varies directly with the minimum perceptibile[21] at that level.

(b) Voigt (*27*, 344; cf. p. 41, above) used three jars, placed in a row side by side, number 1 being on the left. The first and second ones were used for adaptation and always differed by either 5° or 10°, neither more nor less (number one being, say, 20°, number two 25°). The two hands were simultaneously immersed up to the wrist ("bis zum Knöchel"), the left hand in jar 1, the right in jar 2, until adaptation was complete or, in any event, satisfactory to him. He then conveyed the left hand to jar 2 and the right to jar 3, whose temperature was thereupon adjusted until it was subjectively equal to that of the left hand in jar 2. Thus, in our example, the left hand passes from 20° to 25°, experiencing an increment of 5°; now if "perfect" adaptation were possible, he says, "so müssten zwei Hände, welche auf die Temperaturen von 20 und 25 Grad adaptiert sind, den Unterschied zwischen 25 und 30 oder 30 und 35 Grad gar nicht empfinden und erst Unterschiede von mehr als 5 Grad deutlich wahrnehmen" (p. 352); that is, in our case, 30° to the right hand should seem equal to 25° to the left. But he found instead that when jar 3 is only 26° it already seems equal to the 25° in jar 2; whence he concludes that adaptation at this level is only 1/5 or 20% of its "perfect" or theoretical value. The following table gives the percentages so obtained for the several temperatures listed:

	Jar		*Degree of*
	Left	*Right*	*adaptation*
1.	10°	15°	.10
2.	15°	20°	.10
3.	35°	40°	.20
4.	40°	45°	.20
5.	10°	20°	.10
6.	30°	20°	.15
7.	35°	25°	.25
8.	35°	45°	.15
9.	45°	35°	.20

[21] As above noted, (p. 56), this term is employed merely as a convenient designation, with no implications whatever about the nature of adaptation.

Of these data Voigt says, "Eine deutliche Gesetzmässigkeit ist in diesen Zahlenreihen nicht zu erkennen" (p. 354); the figures all prove, however, that the degree of possible adaptation is "auffallend gering", less than we ordinarily suppose; but in his opinion a larger adaptative surface would "wahrscheinlich" give higher percentages.

We may easily throw these figures into a form which will reveal their true and rather striking Gesetzmässigkeit. Let us take the mean ratio, for each 5°-interval from 5° to 45°, of all the determinations listed; thus for the range 35°-40°, the ratio in 3 is .20, in 8 is .15, in 9 is .20, whence the mean is .18. We then have:

10°	15°	20°	25°	30°	35°	40°	45°
.10	.10	.15	.20	.25	.18	.18	

Adaptation, we now see, is most complete in the region of skin-temperature (30°-35°) and falls off in both directions with a uniformity that is specially noteworthy below that region. The trend again is almost linear and inverse to our own limens in A, 1; whence we may set up another proposition: The degree of possible adaptation (which we propose to denominate hereafter the "adaptive index") at a given temperature-level (1) varies inversely with its departure from skin-temperature; (2) varies inversely with the minimum perceptibile at the same level.

With the aid of these facts we are now ready to examine, and, if we may, explain in terms of adaptation, the two forms of curve which appear in connection with our two modes of adaptation (adaptation to 32° and to each J in turn).

Consider, first of all, the curves in A, 1, A, 11, and A, 2, where the receptive area is adapted to each J in turn and where the limens rise by uniform increments. Let the judging surface be adapted as completely as possible to a standard of 36°. If we now go from 36.00° to 36.15°, the receptive mechanism begins adapting at once to the new temperature; inasmuch as the adaptive index here is less than at 32°, however (.18 instead of .25), a larger objective (stimulus-) difference is needed to give the same subjective effect (minimal degree of perceptibility) as does a smaller increment at 32°. We have just seen, in Voigt's data, that the adaptive index falls off quite uniformly on either side of skin-temperature; if dis-

crimination were a function of this index, therefore, we should expect the perceptible minima to increase with correlative uniformity. But the phenomenon of Weber's law, as our curves show, is somehow a function of adaptation; and may therefore with reason be ascribed (or, in any event, related) to the changes of the adaptive index.

In other words, by our theory, if A be the adaptive index and D the stimulus-difference, their product (AD) is, roughly, a constant. The two vary reciprocally; as one rises, the other falls. This product (AD) gives the actual amount of adaptation or adjustment which the organism undergoes in a given case; it is about the same for a liminal stimulus at 28° as at 16°; and because of this relation *the absolute limen becomes an inverse measure of the organism's capacity for adjustment on a given level.*

We now face the correlative question: why do the liminal curves take on a sharply negative acceleration when we adapt to 32° throughout? In the case just considered above, we begin with a receptor adapted as fully as may be to a given temperature (say 20°); the stimulus D is then merely the difference between S (the standard or adaptation temperature) and J (say, 20.00 and 20.15). Here, on the contrary, the degree of stimulation, to which the receptive mechanism is exposed and thereby required to adjust during the process of comparison, is much greater, including not merely the small differences of S and J but the whole interval as well from skin-temperature to the one in question (32° and 20°). The total shift or change of adaptation which the organism can or does undergo, varies with the magnitude of this interval; the adaptive adjustment is obviously greater from 32° to 20° than from 32° to 28°. Is there, however, any means of determining, with reasonable precision, the *net actual displacement of adaptive level* when exposed to a given degree of stimulation? With the aid of Voigt's results, as above tabulated for each 5°-interval from 5° to 45°, this can be done as follows: At 32°, A = .25 but D we assume to be zero, whence AD = .00. From 32° to 30°, A remains .25, whence the adaptive level shifts .50° (.25 of 2°) in that interval; from 30° to 28°, A is .20 and the net change of adaptation is .40°; the total displacement from 32° to 28° thus turns out to be .90°. By this procedure we reach the following table:

Value of AD for the temperatures listed.

32°	(.25×0)	=	.00°
28°	.00° + (.25×2) + (.20×2)	=	.90°
24	.90 + (.20×3) + (.15×1)	=	1.65
20	1.65 + (.15×4)	=	2.25
16	2.25 + (.10×4)	=	2.65
36	.00 + (.25×3) + (.18×1)	=	.93
40	.93 + (.18×4)	=	1.65
44	1.65 + (.18×4)	=	2.37

If we now fit our usual quadratic equation ($y = a + bx + cx^2$) to these figures for AD (net amount of adaptive adjustment) from 32° to 20°, and from 32° to 44°, we have:

32°-20° $y = .0000° + .2438°x - .0047°x^2$.
32°-44° $y = .0145° + .2330°x - .0031°x^2$.

Do these two equations help to explain the course of our own limens in B, 2, B, 11, B, 12, with their negative acceleration?

Let the receptive mechanism be adapted to 32°, and then exposed for two seconds, in one case to a pair of stimuli (S and J) near 20° and again to a pair near 24°. In either case, the organism begins adapting at once to the interval of 12° or of 8°, but the process is interrupted after just two seconds. Since an observer's judgment somehow depends upon adaptation, as we have already shown, there must be something in the rate or intensity of the adaptive process during this two-second exposure to S and to J, that apprizes O of the disparity in objective stimulation; the character of adaptation within this period can alone help determine the response. And so we start the question: How is adaptive rate within the exposure-period related to the figures for complete adaptability (AD), which are set down above? Adaptation clearly proceeds with greater intensity or speed, during the initial two seconds, upon exposure to 20° than to 24° or 28°; but how much greater?

In reply, we find it natural to assume that the adaptive rate during this uniform initial period will be in constant ratio with the total adaptive change (AD) of which the organism is capable on the various levels. As we move away from 32° in either direction, the degree of adaptive displacement in the first two seconds may be assumed to increase in step with, to remain a constant fraction of, the total adjustment possible in these temperatures.

Why do we find it "natural" to concede this proposition? For three reasons: (1) we can prove that adaptive rate tends to accelerate negatively on either side of 32°; (2) by Weber's law we assume the limens to increase pari passu with adaptive rate; (3) we can prove that liminal increase away from 32° agrees very well indeed with the figures for AD. There is here but one real assumption, (2); and that, as we shall see, is easily the most likely one possible.

Let us consider the reasons in turn:

(1) Adaptive rate tends to accelerate negatively on either side of 32°. The mean rate (R) of cooling is given with the equation

$$R = \frac{AD}{t},$$

where A is the adaptive index, D is the stimulus (in- or efflux of heat energy), and t is the time required for complete adaptation (Holm). As we depart from adaptation-temperature (32°), these three variables change as follows: (a) D increases in direct ratio with the distance from 32°. Newton's law of cooling, which is accurate enough within our limits, is thus given by Draper (*9*, 219): "The rate of rise or fall of temperature of a body is proportional to the difference between the temperature of the body and that of the enclosure;" that is, at 4° below 32° the heat-efflux during two-seconds exposure would be k, at 8° below 2k, at 12° below 3k, and so on.[22] Now, if both A and t were constant, R would also increase uniformly and keep a constant ratio with D, the loss or gain of heat; but (b) as Voigt's data prove, the adaptive index (A) falls off rather sharply on either side of 32°; the organism becomes less and less able to cope with temperatures as they depart more and more from that level. Furthermore (c) the time needed for complete adaptation (t) increases on either side of 30° (Holm; above, p. 105). This may well have less effect during the initial exposure-period than later, but in any

[22] It may be that heat-interchange with the skin does not precisely follow Newton's law. The devices for cutaneous and vasomotor constriction and dilation, by which the organism is shielded from too rapid loss and gain of heat, may well make the rate of exchange in extreme temperatures *less* than the law of cooling would lead us to assume. In any event, this is bound up with the whole adaptive process; and, if true, may well be a principal cause for the decrease of adaptive index near the extremes.

event would serve to decrease the adaptive rate. For both reasons therefore (rise of t and fall of A), the adaptive rate departs more and more from that uniform increase which the law of cooling would lead us to postulate.[23]

(2) If Weber's law holds, the limens will increase pari passu with adaptive rate. Taking a pair of stimuli (S and J) near 24°, we have two adaptive processes of slightly unequal intensity or rate; a certain liminal difference in stimulation is needed to make this inequality perceptible; but with a pair at 20°, where adaptation during the exposure-period is more intensive, a *proportionably greater* liminal difference, by Weber's law, would be required to make the two adaptive processes distinguishable. Even though the limens themselves reveal no trace of Weber's law, it seems logical to assume a constant ratio operative here. While we cannot prove a uniform ratio of limen to adaptive rate, the assumption, in our opinion, is by far the most plausible we can make.[24]

(3) The successive limens on either side of 32° increase in step with the correlative values of AD (net amount of adaptive adjustment). From casual inspection, the two equations above given clearly suggest, that both Warm and Cold limens increase at a mean rate which is quite similar to the course of AD. But let us scrutinize more nearly the two sets of equations (for AD, and for the observed limens). We should not expect the coefficient of x^2, which reveals the degree of negative curvature, to agree closely in the two groups of equations, for several reasons: (1) In our own liminal curves, the actual degree of negative acceleration varies widely with differing observers and methods; (2) The coefficients for warm and cold limens, in the mean, differ substantially, as noted above p. 103); (3) Voigt himself emphasizes the provisional and approximative character of his figures; (4) owing to practise effects and limited array of observations, a few of our own curves by method B have but limited reliability. Withal, we find the coefficients of x^2 in the two groups of material comparable in magnitude, as will appear from the table:

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[23] The above argument applies to the *mean* rate of adaptation; but the same factors, of course, operate during the first two seconds; though, as the rest of our analysis will show, in smaller degree.

[24] The logical credibility of this conclusion is clearly shown in the writer's article (*7c*).

			Limens		
	AD	*W*	*C*	*W+C*	*Mean*
32°-20°	.0046	.0110	.0044	.0154	.0077
32°-44°	.0031	.0046	.0037	.0083	.00415
		.0049	.0004	.0053	.00265
		.0157	—.0006	.0151	.00755
				.0021	.00105
				.0015	.00075
				.0018	.0009
				.0061	.00305
				General mean	.0029

While .0046 departs a good deal from .0077, we have already seen (p. 74) that the figure .0154 is too large: one of the limina is clearly excessive and in two of them practise effects continue; the mean .0077 is therefore much beyond the true value and its deviation from .0046 cannot weigh heavily with us. The mean .0029, on the contrary, which is derived from seven individual determinations, agrees surprisingly with the AD-value, .0031. In view of the above reasons for expecting inequality in the two sets of figures, we may conclude that they agree very well indeed.

We have now shown that adaptive rate tends to accelerate negatively; that the limens, presumably, increase pari passu with adaptive rate; and that the successive values of AD, finally, increase in step with the correlative limens. We believe it natural, therefore, and reasonable to adopt the proposition we set out to establish: that adaptive rate during the period of exposure varies in direct ratio with the whole possible amount of adaptation (AD) in that temperature.

To sum up: our data prove that adaptive conditions are somehow responsible for the two forms of liminal increase, rectilinear and negatively accelerated; we have therefore sought to explain them in adaptive terms—the concept AD (the degree of adjustment which the organism can effectuate on a given level) or the adaptive rate, which, as we have seen, varies in step with AD. When we are adapted to a given temperature, say 20°, the minimum perceptibile or absolute limen is identical with and constitutes the whole stimulation (D), and needs must increase as the adaptive index (A) falls, if their product (AD), or its correlative adaptive rate, is to evoke the same degree of response from the organism (minimal perceptibility). We thus have the uniform increments

which characterize the one group of curves (adaptation to each J in turn). When we are adapted to 32°, however, the liminal difference is but a small part of the total stimulation, and now increases in step with the adaptive rate, that is, with the product AD; whence the limens accelerate negatively on either side of the adaptation-level, 32°.

We are now ready to ask why the cold limens, in method B, depart from a linear increase less than do the warm ones. We have already seen that the rise in successive limens is a matter of adaptation. This being true, we may assume that the two types of liminal curve would be more unlike where adaptability is high than when it is small. The more a given sensory function is amenable to adaptation, the more its DL are likely to be affected by a change in adaptive conditions. But the cold limens, as we have seen, are much less affected by variant adaptive conditions than are the warm; whence we conclude that the sensory mechanism for cold is less adaptable, less easily and widely adjustable than is the mechanism for warm. If this proposition be true, we have a fact of wide reaching consequence, in our opinion, for any theory of the thermal senses. We have already given our view that a relation exists between cold and pain, more intimate than we now suspect; and the above conclusion seems to show that cold, like pain, is little subject to adaptive influences. The common impression that cold is highly adaptable seems, indeed, erroneous. The decline of a cold sensation is obviously due, in large and perhaps major degree, not to a change in the receptive or central mechanism, but to a decrease of intensity in stimulation; the skin and vessels constrict, and thereby lessen the efflux of heat, whence the cause of the cold impression is greatly or wholly reduced. It may well be, therefore, that the cold response itself, like pain, is little susceptible to adaptive influence.

2. Theory of Thermal Function

Let us now summarize the general theory which we propose, subject to revision, and which may well be applicable to other sense-fields than temperature.

(1) The relation known as Weber's law holds clearly and consistently for absolute limens (minima perceptibilia) but not at all for differential limens (minima distingibilia). This has been amply demonstrated above for thermal discrimina-

tion; a far wider question is thereby laid open and claims respectful attention: whether the Weber ratio, insofar as it appears in other sensory fields, is also a relation between absolute, instead of differential, limens, as heretofore has always been assumed. We here have space merely to broach the question; but we may remind the reader that the matter of adaptation is seldom adequately provided for, if indeed considered at all, in studies of sensory discrimination; furthermore, in some fields, like audition, where adaptability is low, adaptive conditions have relatively small influence upon the course of the liminal curves, as we find with our own cold thresholds above; in such cases, the differential minima tend to increase with approximate linearity even as do the absolute limens. In any event, the whole phenomenon of Weber's law in every field may well be a matter of adaptation and explicable only in adaptive terms; the writer hopes to examine the question in more detail elsewhere.

(2) Whatever else it may be, Weber's law is an inverse function of relative adaptability on successive stimulus-levels, the degree of possible adaptation to a stimulus changing inversely with the perceptible minima. Absolute sensitivity varies positively with the adaptive index: the higher the index, the smaller is the objective change needed to give a perceptible difference. A low absolute threshold goes with high (rapid and complete) adaptability; wherein we are *very adaptable,* therefore, we are *highly* (albeit briefly) *sensitive* for minimal impressions.

(3) The differential limina increase with negative curvature from the point of adaptation and vary directly with adaptive rate, ($\frac{AD}{t}$; this concept is carefully defined above, p.) ; whence the following relations of threshold to adaptation eventuate:

(a) The minimum distingibile varies in step with adaptive rate ($\frac{AD}{t}$) ; as the latter goes up or down, so likewise the former.

(b) Any change in one of these factors will proportionably affect the whole quantity (DA/t), and the limen as well. The differential minimum varies with A: high adaptability now means correlatively high threshold; it varies also with D: increase of stimulation raises the threshold in proportion; it

varies finally with t: as the time required for "complete" adaptation falls, the rate per unit of time rises, and with it the limen. Various combined changes are, of course, possible; if A falls, say, when D rises, as in our figures above, the adaptive rate will change more slowly than if either were to remain constant.

(4) The *more* easily and completely *adaptable* a receptive mechanism is, therefore, the *smaller* the absolute limen (minimum perceptibile) and the *greater* the differential (minimum distingibile).

(5) The adaptive rate (speed of adjustment) thus becomes a direct measure of "sensation-intensity." What is known as the quantitative attribute of experience is fixed and may be measured by the rate with which the organism is prepared to adapt to its stimulus.

(6) When exposed to stimulation, the organism at once begins what is commonly known as a process of "adjustment" leading to eventual "adaptation." This change is attended by what is called a "quantitative" alteration in the quality or mode of experience which began with the incipience of the stimulus and now persists in some degree as long as the adjustive process continues. *Any form of sensation,* therefore, is a mark or sign that we are *ill-adjusted, mal-adapted;* only when it stops have we achieved a measure of equilibrium or repose whence we are relieved of further efforts at accommodation. The adaptive index provides a measure of the organism's ability to cope with a given mode of external change; if the index be high, we know it is well "adapted" to meet that form of situation. Mental life, so far as it depends on the reception of stimuli, is made up of these adjustive processes, one relieving another and few coming to completion. Whatever else be true of a limen, therefore, it reveals how much a given situation need be changed to initiate a new effort of accommodation on the part of the responsive mechanism.

CHAPTER VIII

Appendix: Tables of Data and Bibliography

1. Criteria for Acceptance of Data

In making up, from the protocols, the final summaries to which statistical treatment was applied, the writer broached a problem of no little consequence, to which careful consideration was devoted. Inasmuch as the temperature of the same jar usually underwent changes of one or more hundredths degree in the course of an afternoon, as revealed by successive readings or checks, the two-fold question arose:

1) What temperature shall be assumed for an hour in which no reading was taken?

2) How widely may the temperature of a jar vary from one reading to the next, and still be accepted as reliable?

The answer to question 1) is fairly obvious, and was uniformly applied in all cases: The temperature for any hour is to be derived by linear interpolation between the readings next before and after.

Example: If a jar reads 36.10° at 1 p.m. and 36.13° at 4 p.m., the temperature at 3 p.m., by this rule, equals 36.12°.

Question 2) is far more intricate; in answer to it were formulated, and carefully applied throughout the data, the following *Criteria of Admissible Change* in the "constant" temperatures:

a) If the difference between two successive readings of the same jar does not exceed .10°, the value for each intervening hour, as determined by 1) above, is used with full weight.

Example: If a jar reads 36.05° at 1 p.m. and 36.09° at 5 p.m., the interpolated values at 2 p.m. (= 36.06°), 3 p.m. (= 36.07°) and 4 p.m. (= 36.08°) are used with full weight.

b) If the difference between two successive readings of the same jar exceeds .10°, but does not exceed .03° per hour for the time that has elapsed, the value for an hour just before or after either reading is used with full weight; for the second hour before or after is used with half weight.

Example: If a jar reads 36.05° at 1 p.m. and 36.16° at 5 p.m. (where the increase of .11° exceeds .10° but amounts to less than .03° per hour for the period of 4 hours) the values, as found by 1) above, for the hours 1-2 and 4-5 are used with full weight, for the hours 2-3 and 3-4 with half weight.

The two formulas above define the basic limits of admissible change, as used in tabulating our data; in addition were developed the following rules, for handling special cases:

c) If only one reading of a given setting is available, that value is used for the hour just before and after with half weight.

Example: If a certain jar is read only once on a given setting, say, at 3 p.m., that reading is used for the hours 2-3 and 3-4 with half weight; but not at all for any other hours.

d) If the first check-reading of an afternoon agrees with a later one, but not with the scheduled temperature which the jar was to be given that day, then the value for the hour just before the first check is used with half weight.

Example: If a jar, by the original schedule, was to be given the temperature of 36.05°, but, on the first check at 2 p.m., reads 36.17°, the discrepancy of .12° exceeds the limits fixed in a) and b) and would normally justify the rejection of the value before this check; but if a later reading at 4 p.m. confirms the previous one, giving, say, 36.15°, the former value, 36.17°, is used for the preceding hour, 1-2, with half weight.

e) The value for an hour just before (or after) a reading is used with full weight, when confirmed by the next later (or earlier) check and not opposed by the next earlier (or later) check; the value for the second hour before or after, under these conditions, is given half weight.

Example: If a jar, on its first reading at 2 p.m., equals 36.05° (the original temperature, at the time when the jar was set, not having been recorded) and at 5 p.m. equals 36.08°, which confirms the earlier reading; then the 2 o'clock temperature is used for the hour, 1-2, with full weight, and for the hour, 12-1, with half weight.

f) Values are not extrapolated, but are made equal to the nearest reading, so far as used at all.

Example: If a jar rises from 36.05° at 1 p.m. to 36.08° at 4 p.m., a mean rise of .01° per hour, the value for 5 p.m. is not extrapolated to 36.09° on this basis, but is taken the same as the figure at 4 o'clock.

g) Values not strictly conforming to these conditions are rejected.

The above formulas had to be derived from general considerations, there being no definite, empirical means of determining just how much discrepancy, or what margin of possible inaccuracy, may properly be tolerated. The writer tried, however, to make his criteria undeniably conservative; even at the cost, in a few cases, of a good many judgments. Let us consider the rules briefly, in order.

1) As above pointed out (p. 19), the more important changes in the thermostats are gradual rather than abrupt; linear interpolation, therefore, seems to be indicated.

a) Suppose that a jar reads .00° at 12 m. and .10° at 5 p.m.; the interpolated values for the four intervening hours would then be: .02°, .04°, .06°, .08°. Assume further—what is most unlikely and least favorable to our procedure—that the true temperature continued at .00° until just before the second reading, when it rose abruptly to .10°; the discrepancies between the assumed and the true temperature would then be, in order, as above: .02°, .04°, .06°, .08°; the mean being .05°. It can thus be shown, for any interval, that the mean *maximal* discrepancy between an interpolated value and the true one, under the least favorable conditions, will not exceed .05°, and in nearly all cases will be very much less.

b) If a jar rises, for five hours, at the mean rate of .03° per hour, it can be shown, as in the paragraph above, that the mean maximal discrepancy, by the least favorable assumption, between an interpolated and the true temperature, will not exceed .075° and will, in general, be much less. The greater range, however, makes the values less reliable; so that only those for the hour just before or after a reading are given full weight, while those for an hour second before or after are weighted one-half. This, in our opinion, is a conservative procedure.

c) When only one reading of a given setting was recorded, as happened a few times, there was no means of knowing in

what direction or by what rate the thermostat was changing; the values used, therefore, were given only half weight.

d) This rule was devised for a few cases in which the temperature was not set with enough care before work began. If two separate readings, in the course of the afternoon, agreed well with each other but differed widely from the scheduled value, the natural presumption was that the fault lay, not with the thermostat, but in the original setting; the value for the hour preceding the first check was therefore used, though with half weight. It should be noted that, in nearly all cases (cf. p. 23), the temperatures were checked either at the time of setting or before any judgments were taken; this rule was needed only in a few cases where this had not been done.

e) In a few cases the original setting was not recorded. If, in these circumstances, two separate readings during the afternoon agreed well with each other, thereby showing that the thermostats were adequately functioning, it was thought entirely proper to use the value for the hour before the first check at full weight, and for the hour second before with half weight.

f) This rule again applied to few cases but is mentioned for the sake of completeness. It is well recognized that extrapolation, on the basis of a purely empirical curve or course of movement by a variable, is hazardous; there seemed no reason to employ it here, especially since the extrapolated value seldom varied from the one actually used by more than one or two hundredths.

To conclude: the writer needs no admonition that weighting values, whether by zero or one-half, is precarious; but something of the sort was here inevitable. Successive readings of the same jar seldom agree precisely; a thermostat for some reason departs from its normal degree of precision; the absence of a helper deprives the record of some needed checks: the vicissitudes of actual work all combine to give unlike degrees of precision to the data. The writer believes that the above criteria will be accepted as reasonable and conservative; but wishes also, in this connection, to recall and emphasize two facts already given in detail. 1) The high constancy of the thermostats—that is, their small mean change per hour as shown in Table 1; this may serve to prove their general reliability and to justify our use of some values, in accord with

the rules above stated, which had not been checked as completely as desirable. 2) While the number of judgments for temperatures 40° and 44° is undeniably small, owing both to limited time and to the application of our criteria, yet the high general consistency of the data, as shown in Tables 9 and 10, may be taken as evidence that the final results presumably differ but little from the values that a larger number of cases would have yielded.

A few minor details of the tabulating may here be noted:

The temperature of a jar at the close of an hour, whether determined by actual reading or by interpolation, was taken as the temperature for the whole series occupying that hour.

The temperature scheduled for any jar was always an integral multiple of .05°; the tables of data likewise have uniform intervals of .05°. An objective difference ranging from —.02° to +.02° was tabulated as .00°, from .03° to .07° as .05°, and so on. The accuracy of control did not seem to warrant the use of finer intervals.

The judgment "Equal", which almost never appeared after the practise series, was tabulated as "Doubtful."

2. *Data by Culler; Constant Stimulus-Differences*

a. Notes

The several methods (e.g. A, 1) have already been defined; cf. p. 56. For the meaning of "Group I" and "II", see p. 18.

The figures 28°, 24°, and so on, denote the temperatures to which a given entry applies. Under each method, these temperatures are listed in the precise order in which they were worked.

The time-order, S-J, means that the standard stimulus (S) preceded the one to be judged (J); J-S reverses that order. *L* means that J was to left of S; *R*, to the right.

Of the column-heads, h_w is the "measure of precision" of the "most probable" Gaussian ogive that can be fitted to the Warmer-percentages; it equals $\frac{1}{\sigma\sqrt{2}}$; h_c is the same value for the Cooler-percentages. L_w is the crude Warm limen: the median, or abscissal point, where the per cent of Warmers is .50 in the fitted curve; L_c is the corresponding Cold limen. The p.e. of L_w and L_c is derived from the correlative value of

h by the formula: p.e. $= .84535 \frac{1}{h\sqrt{2n}}$. I denotes the "interval of uncertainty", that is, the distance from the warm to the cold limen; $L_w - L_c = I$. The p.e. of this interval is found by the usual formula for two independently measured quantities: $p.e._I = \sqrt{p.e.^2 L_w + p.e.^2 L_c}$. Xi is the "point of subjective equality" (Urban), where the probability of a W-judgment just equals that of a C-judgment. It is here taken as the boundary dividing Warm and Cold, the point from which the "true" warm and cold limens are to be reckoned. The true value for the warm limen thus equals $L_w - Xi$, for the cold limen $L_c - Xi$. No method of calculating the p.e. of Xi has, to our knowledge, been published; so that its p.e., as well as those for the true limens, is omitted. N, finally, gives the number of trials or judgments upon which the entries of a given row are based.

b. TABLES

Method: A, 1.

Group I		h_w	h_c	L_w	PE	L_c	PE	I	PE	X_i	*"True" Limens* L_w	*"True" Limens* L_c	N
32°	S-J L	7.30	— 6.36	—.083°	.015°	—.241°	.017°	.158°	.023°	—.157°	.074°	—.084°	340
	R	8.56	— 6.28	.008	.015	—.193	.026	.201	.030	—.077	.085	—.116	340
	Mean			—.037	.011	—.117	.016	.180	.019	—.117	.080	—.100	680
28°	S-J L	10.62	— 3.70	.038	.014	—.210	.044	.248	.046	—.026	.064	—.184	144
	R	7.90	— 9.32	.016	.019	—.106	.018	.122	.026	—.050	.066	—.056	144
	J-S R	4.92	—14.66	.121	.039	—.037	.011	.158	.040	.003	.118	—.040	112
	L	7.00	— 5.16	.094	.025	—.035	.032	.129	.041	.039	.055	—.074	112
	Mean			.067	.013	—.097	.015	.164	.020	—.009	.076	—.088	512
24°	S-J L	7.34	— 5.56	.046	.017	—.052	.023	.098	.029	.004	.042	—.056	234
	R	6.12	— 5.72	.035	.022	—.089	.024	.124	.033	—.025	.060	—.064	221
	J-S R	4.32	— 8.74	.064	.033	—.074	.017	.138	.037	—.028	.092	—.046	164
	L	5.94	— 6.12	.123	.026	+.004	.023	.119	.035	.063	.060	—.059	164
	Mean			.067	.013	—.053	.011	.120	.017	.003	.064	—.056	783
20°	S-J L	10.64	— 5.48	.058	.011	—.027	.024	.085	.027	.029	.029	—.056	164
	R	7.12	— 5.94	.070	.019	—.046	.022	.116	.029	.017	.053	—.063	164
	J-S R	5.70	— 4.26	.034	.020	—.078	.032	.112	.037	—.014	.048	—.064	252
	L	4.62	— 4.76	.050	.024	—.074	.024	.124	.034	—.013	.063	—.061	270
	Mean			.053	.010	—.056	.013	.109	.016	.005	.048	—.061	850
16°	S-J L	6.16	— 4.46	.179	.020	+.019	.027	.160	.034	.112	.067	—.093	180
	R	4.44	— 5.38	.040	.028	—.085	.022	.125	.036	—.028	.068	—.057	179
	J-S R	6.40	— 4.52	—.034	.019	—.185	.030	.151	.036	—.097	.063	—.088	175
	L	5.64	— 6.02	.059	.024	—.075	.022	.134	.033	—.010	.069	—.065	172
	Mean			.061	.012	—.082	.013	.143	.017	—.006	.067	—.076	706

Method: A, 1.

Group I											*"True" Limens*		
		h_w	h_c	L_w	PE	L_c	PE	I	PE	X_i	L_w	L_c	N
20°	S-J L	4.54	— 5.04	.105°	.024°	—.016°	.020°	.121°	.031°	.042°	.063°	—.058°	316
	R	4.12	— 4.78	.042	.023	—.087	.024	.129	.033	—.027	.069	—.060	303
	J-S R	5.82	— 5.32	—.011	.014	—.111	.019	.100	.023	—.059	.048	—.052	383
	L	6.22	— 6.34	.055	.014	—.054	.012	.109	.019	.000	.055	—.054	389
	Mean			.048	.010	—.067	.010	.115	.014	—.011	.059	—.056	1391
24°	S-J L	7.12	— 7.12	.086	.013	—.012	.014	.098	.019	.037	.049	—.049	240
	R	7.32	— 8.42	.017	.013	—.068	.011	.085	.017	—.028	.045	—.040	240
	J-S R	9.20	— 6.34	—.031	.010	—.103	.018	.072	.021	—.061	.030	—.042	225
	L	8.52	— 7.48	.037	.011	—.032	.012	.069	.017	.005	.032	—.037	225
	Mean			.027	.006	—.054	.007	.081	.009	—.012	.039	—.042	930
28°	S-J L	7.68	— 4.84	.132	.025	+.057	.035	.075	.043	.103	.029	—.046	100
	R	4.28	— 4.56	.069	.039	—.054	.040	.123	.055	.006	.063	—.060	100
	J-S R	5.10	— 8.84	—.016	.035	—.102	.018	.086	.040	—.070	.054	—.032	90
	L	8.64	— 6.54	.014	.017	—.069	.025	.083	.030	—.022	.036	—.047	94
	Mean			.050	.015	—.042	.015	.092	.022	.004	.046	—.046	384

Method: A, 1.

Group II											*"True" Limens*		
		h_w	h_c	L_w	PE	L_c	PE	I	PE	X_i	L_w	L_c	N
44°	S-J L	2.88	— 2.28	.057	.055	—.167°	.087°	.224°	.103°	—.042°	.099°	—.125°	105
	R	3.42	— 2.78	.177	.053	—.012	.059	.189	.079	.093	.084	—.105	105
	J-S R	0.30	— 2.98	.807	.679	+.140	.058	.667	.681	.201	.606	—.061	132
	L	3.74	— 3.14	.078	.040	—.033	.052	.111	.065	.027	.051	—.060	132
	Mean			.148		—.018	.033	.166		.070	.078	—.088	474
40°	S-J L	1.98	— 1.18	—.129	.133	—.307	.214	.178	.252	—.196	.067	—.111	62
	R	3.86	— 5.10	.023	.062	—.150	.031	.173	.070	—.075	.098	—.075	62
	J-S R	2.96	— 5.42	.161	.054	+.009	.028	.152	.061	.063	.098	—.054	132
	L	5.08	— 4.80	.073	.030	—.048	.028	.121	.041	.014	.059	—.062	132
	Mean			.032	.040	—.124	.055	.156	.068	—.049	.081	—.075	388
36°	S-J L	7.04	— 9.18	—.040	.018	—.140	.016	.100	.024	—.097	.057	—.043	180
	R	6.02	— 6.06	.006	.020	—.153	.021	.159	.029	—.074	.080	—.079	180
	J-S R	7.48	— 7.22	.135	.015	+.023	.014	.112	.021	.080	.055	—.057	214
	L	7.36	—10.58	.068	.014	—.030	.009	.098	.017	.010	.058	—.040	218
	Mean			.042	.008	—.075	.008	.117	.012	—.020	.062	—.055	792
32°	S-J L	10.94	— 7.22	—.026	.009	—.110	.014	.084	.016	—.059	.033	—.051	150
	R	9.00	— 9.88	—.009	.011	—.064	.011	.055	.015	—.038	.029	—.026	152
	J-S R	6.42	—11.94	.050	.016	—.015	.009	.065	.018	.008	.042	—.023	154
	L	8.64	— 9.66	.072	.014	—.040	.010	.112	.018	.013	.059	—.053	154
	Mean			.022	.006	—.057	.006	.079	.008	—.019	.041	—.038	610
28°	S-J L	9.16	— 7.96	.015	.010	—.054	.013	.069	.017	—.017	.032	—.037	152
	R	5.36	— 9.74	.030	.020	—.046	.009	.076	.022	—.019	.049	—.027	152
	J-S R	9.26	—15.96	.024	.010	—.039	.006	.063	.012	—.016	.040	—.023	149
	L	12.30	— 9.14	.025	.007	—.051	.012	.076	.014	—.007	.032	—.044	154
	Mean			.023	.006	—.048	.005	.071	.008	—.015	.038	—.033	607

Method: A, 2.

Group I		hw	hc	Lw	PE	Lc	PE	I	PE	Xi	*'True' Limens* Lw	Lc	N
28°	L	4.16	— 6.02	.049°	.025°	—.127°	.023°	.176°	.034°	—.055°	.104°	—.072°	300
	R	5.18	— 4.56	.083	.025	—.190	.022	.273	.033	—.045	.128	—.145	300
	Mean			.066	.018	—.159	.016	.225	.024	—.050	.116	—.109	600
24°	L	4.60	— 5.02	.014	.025	—.164	.024	.178	.035	—.079	.093	—.085	252
	R	4.62	— 4.40	.094	.027	—.106	.031	.200	.041	—.004	.098	—.102	235
	Mean			.054	.018	—.135	.020	.189	.027	—.041	.095	—.094	487
20°	L	6.66	— 5.66	.038	.016	—.085	.018	.123	.024	—.019	.057	—.066	290
	R	3.30	— 4.20	.098	.031	—.063	.026	.161	.040	.008	.090	—.071	312
	Mean			.068	.017	—.074	.016	.142	.024	—.005	.073	—.069	602
16°	L	4.58	— 3.72	.113	.034	—.045	.043	.158	.055	.042	.071	—.087	110
	R	3.14	— 3.52	.010	.055	—.146	.043	.156	.070	—.072	.082	—.074	113
	Mean			.062	.032	—.095	.030	.157	.044	—.015	.077	—.080	223
20°	L	3.48	— 2.62	.122	.019	—.094	.026	.216	.032	.029	.093	—.123	485
	R	4.38	— 5.90	—.014	.015	—.136	.011	.122	.019	—.084	.070	—.052	481
	Mean			.054	.012	—.115	.014	.169	.019	—.028	.082	—.087	966
24°	L	5.30	— 4.74	.072	.026	—.080	.026	.152	.037	.000	.072	—.080	158
	R	6.04	— 6.52	.002	.020	—.110	.020	.112	.028	—.056	.058	—.054	165
	Mean			.037	.016	—.095	.016	.132	.023	—.028	.065	—.067	323

Method: A, 2.

Group II		hw	hc	Lw	PE	Lc	PE	I	PE	Xi	*"True" Limens* Lw	Lc	N
40°	L	2.60	— 5.36	—.006°	.035°	—.164°	.017°	.158°	.039°	—.112°	.106°	—.052°	330
	R	4.68	— 4.62	.176	.021	+.033	.018	.143	.028	.105	.071	—.072	330
	Mean			.085	.020	—.065	.012	.150	.024	—.003	.088	—.062	660
36°	L	5.28	— 4.54	.004	.019	—.138	.023	.142	.030	—.062	.066	—.076	282
	R	4.92	— 4.68	.125	.018	—.016	.020	.141	.027	.056	.069	—.072	282
	Mean			.065	.013	—.077	.015	.142	.020	—.003	.068	—.074	564
32°	L	6.24	— 5.40	.050	.016	—.080	.016	.130	.022	—.011	.061	—.069	324
	R	7.30	— 6.38	.065	.011	—.055	.014	.120	.018	.009	.056	—.064	333
	Mean			.058	.010	—.067	.011	.125	.014	—.001	.059	—.066	657
28°	L	2.84	— 3.14	.130	.033	—.106	.030	.236	.044	.006	.124	—.112	330
	R	1.68	— 2.42	.012	.057	—.241	.043	.253	.071	—.137	.149	—.104	329
	Mean			.071	.033	—.173	.026	.244	.042	—.065	.136	—.108	659

Method: A, 3.

Group I		hw	hc	Lw	PE	Lc	PE	I	PE	Xi	*'True' Limens* Lw	Lc	N
32°	Left L	6.90	— 5.10	—.066°	.025°	—.289°	.036°	.223°	.044°	—.161°	.095°	—.128°	124
	Hand R	7.66	— 5.50	—.019	.022	—.168	.031	.149	.038	—.082	.063	—.086	124
	Mean			—.043	.017	—.229	.024	.186	.029	—.122	.079	—.107	248
	Right L	7.14	— 1.22	—.022	.025	—.454	.168	.432	.170	—.085	.063	—.369	122
	Hand R	4.76	— 0.74	—.004	.036	—.359	.223	.355	.226	—.052	.048	—.307	122
	Mean			—.013	.022	—.407	.140	.394	.141	—.069	.056	—.338	244

Method: B, 2.

Group I		h_w	h_c	L_w	PE	L_c	PE	I	PE	X_i	*'True' Limens* L_w	*'True' Limens* L_c	N
32°	L	5.24	— 5.38	.072°	.027°	—.185°	.028°	.257°	.039°	—.058°	.130°	—.127°	144
	R	8.88	— 5.40	.135	.015	—.118	.030	.253	.034	.039	.096	—.157	144
	Mean			.104	.015	—.151	.021	.255	.026	—.009	.113	—.142	288
28°	L+R			.439	.155	—.268	.128	.707	.201	.053	.386	—.321	524
24°	L+R			1.506	.453	—.481	.198	1.986	.494	.126	1.380	—.606	480
20°	L+R			.461	.092	—.446	.111	.907	.144	.052	.409	—.498	1012

Method: B, 2.

Group II		h_w	h_c	L_w	PE	L_c	PE	I	PE	X_i	*'True' Limens* L_w	*'True' Limens* L_c	N
48°	L	2.68	— 2.90	—.021°	.042°	—.246°	.041°	.225°	.059°	—.138°	.117°	—.108°	282
	R	2.82	— 3.62	.259	.041	—.007	.030	.266	.051	.110	.149	—.117	282
	Mean			.119	.029	—.127	.025	.246	.039	—.014	.133	—.113	564
44°	L			.017	.117	—.149	.106	.166	.158	—.069	.086	—.080	60
	R			.492	.136	—.051	.152	.542	.204	.232	.260	—.283	60
	Mean			.254	.090	—.100	.093	.354	.129	.082	.173	—.182	120
40°	L			.050	.216	—.892	.277	.942	.351	—.364	.414	—.528	96
	R			.225	.094	—.168	.137	.393	.166	.065	.160	—.233	96
	Mean			.138	.118	—.530	.155	.668	.194	—.150	.287	—.381	192
36°	L	0.92	— 1.32	—.098	.164	—.634	.143	.536	.218	—.414	.316	—.220	212
	R	1.96	— 1.82	.401	.087	.052	.077	.349	.116	.233	.168	—.181	210
	Mean			.152	.093	—.201	.081	.443	.123	—.090	.242	—.201	422
32°	L	6.08	— 7.58	.111	.018	—.005	.012	.116	.022	.046	.065	—.051	226
	R	5.90	— 2.64	—.007	.017	—.160	.037	.153	.041	—.055	.048	—.105	226
	Mean			.052	.012	—.083	.019	.135	.023	—.005	.057	—.078	452

3. Data by Other Observers; Method of Limits

a. Notes

All the data in the four tables following were secured by some form of the method of limits (A, 11, B, 11, B, 12); these terms have been defined above (p. 56).

With observers Kraeuter and Lively, the adaptation-temperature is, in every case, 32°; with Geissler and Mulvey, it is given in parenthesis after the temperature of the standard.

Under L_w is given the uncorrected warm limen, under L_c the cold one, under I the interval between them ($L_w - L_c$). *L* means that the variable (to which every judgment applies) was to left of the standard for the entries in that row; *R*, to the right. The symbol means a descending series, the reverse symbol ↑ an ascending one. *M* gives the mean of each pair of values directly above (either L and R, or ↓ and ↑), the two means being the same. With each mean is given the number of cases on which it is based (as, N = 32). In general, the cases are equally divided between L and R; a few times one slightly exceeds the other.

b. TABLES

OBSERVER: GEISSLER

Method: A, 11.

	32° (32°)			36° (36°)			40° (40°)			44° (44°)		
	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc
L	.044°	.153°	—.109°	.152°	.193°	—.041°	.119°	.227°	—.108°	.057°	.287°	—.230°
R	.042	.149	—.107	.134	.180	—.046	.176	.241	—.065	.159	.248	—.089
↓	.028	.149	—.121	.090	.164	—.074	.045	.207	—.162	.014	.243	—.257
↑	.059	.152	—.094	.196	.209	—.013	.251	.261	—.010	.230	.292	—.062
		N=48			N=32			N=32			N=32	
M	.043	.151	—.108	.143	.186	—.043	.148	.234	—.086	.108	.267	—.159

Method: B, 11.

	36° (32°)			40° (32°)			44° (32°)			48° (32°)		
L	.101	.290	—.189	.328	.347	—.019	—.555	.326	—.881	—1.722	.464	—2.186
R	.031	.285	—.254	.225	.360	—.135	—.521	.393	—.914	—1.442	.456	—1.898
↓	.080	.290	—.210	.198	.319	—.121	—.612	.311	—.923	—1.632	.479	—2.111
↑	.052	.285	—.233	.355	.388	—.033	—.464	.408	—.872	—1.532	.442	—1.974
		N=32			N=16			N=32			N=32	
M	.066	.287	—.221	.276	.353	—.077	—.538	.360	—.898	—1.582	.460	—2.042

OBSERVER: MULVEY

Method: A, 11.

	32° (32°)			36° (36°)			40° (40°)			44° (44°)		
	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc
L	.058°	.085°	—.027°	.162°	.125°	+.037°	.184°	.145°	.039°	.146°	.174°	—.028°
R	.064	.084	—.020	.205	.146	.059	.226	.135	.091	.236	.216	.047
↓	.020	.094	—.074	.142	.123	.019	.152	.139	.013	.105	.199	—.094
↑	.102	.074	+.028	.225	.148	.077	.258	.141	.117	.304	.191	.113
		N=32			N=32			N=32			N=29	
M	.061	.084	—.023	.183	.135	.048	.205	.140	.065	.204	.195	.009

Method: B, 11.

	36° (32°)			40° (32°)			44° (32°)			48° (32°)		
L	.181	.161	.020	—.101	.163	—.264	—.407	.171	—.578	—.494	.484	—.978
R	.258	.190	.068	.126	.206	—.080	—.294	.198	—.492	—.374	.460	—.834
↓	.198	.144	.054	—.015	.160	—.175	—.424	.162	—.586	—.521	.442	—.963
↑	.242	.208	.034	.040	.209	—.169	—.278	.206	—.484	—.347	.502	—.849
		N=28			N=24			N=32			N=22	
M	.220	.176	.044	.013	.185	—.172	—.351	.184	—.535	—.434	.472	—.906

OBSERVER: KRAEUTER

Method: B, 12.

	32°			36°			40°			44°			48°		
	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc
L	.061°	.121°	—.060°	.439°	.430°	+.009°	.675°	.449°	+.226°	.286°	.542°	—.256°	.441°	.565°	—.122°
R	.098	.109	—.011	.096	.556	—.460	—.021	.467	—.488	.115	.525	—.410	.274	.377	—.103
↓	.155	.111	+.044	.366	.467	—.101	.334	.331	+.003	.280	.434	—.154	.521	.486	+.035
↑	.004	.119	—.115	.170	.520	—.350	.319	.584	—.265	.120	.632	—.512	.194	.455	—.261
		N=32			N=30			N=27			N=32			N=16	
M	.079	.115	—.036	.268	.493	—.225	.327	.458	—.131	.200	.533	—.333	.358	.471	—.113

Method: B, 11.

	32°			36°			40°			44°			48°		
L	.027	.084	—.057	.014	.267	—.253	—.069	.347	—.416	—1.051	.431	—1.482	—2.442	.276	—2.719
R	.031	.073	—.042	—.070	.256	—.326	—.145	.360	—.505	—1.041	.442	—1.483	—2.091	.254	—2.345
↓	.079	.084	—.005	.074	.258	—.184	—.067	.331	—.398	—.906	.539	—1.445	—2.305	.204	—2.509
↑	—.020	.073	—.093	—.131	.264	—.395	—.147	.376	—.523	—1.187	.334	—1.521	—2.229	.326	—2.555
		N=32			N=28			N=25			N=18			N=16	
M	.029	.078	—.049	—.028	.262	—.290	—.107	.353	—.460	—1.046	.437	—1.482	—2.267	.265	—2.532

OBSERVER: LIVELY

Method: B, 12.

	32°			36°			40°			44°			48°		
	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc	Lw	I	Lc
L	.061°	.252°	—.191°	.343°	.893°	—.550°	.363°	.746°	—.383°	.142°	1.654°	—1.512°	.359°	1.342°	—.983°
R	.064	.229	—.165	1.015	.791	+.224	1.585	1.136	+.449	.874	1.280	— .406	.915	.988	—.073
↓	.104	.257	—.153	.972	.866	+.106	1.370	.805	+.565	.822	1.237	— .415	.990	1.095	—.105
↑	.021	.224°	—.203	.386	.818°	—.432	.578	1.076	—.498	.194	1.697	—1.503	.284	1.236	—.952
		N=32			N=28			N=26			N=28			N=14	
M	.062	.240	—.178	.679	.842	—.163	.974	.941	+.033	.508	1.467	— .959	.637	1.165	—.528

Method: B, 11

	32°			36°			40°			44°			48°		
L										— .999	.465	—1.464			
R	.046	.153	—.107	.059	.498	—.439	—.160	.609	—.769	—1.055	.476	—1.531	—1.132	.432	—1.564
↓	.038	.170	—.132	—.026	.378	—.404	—.058	.649	—.707	—1.004	.418	—1.422	— .974	.302	—1.275
↑	.045	.147	—.102	.033	.405	—.372	—.135	.561	—.696	—1.050	.522	—1.572	—1.150	.366	—1.515
	.039	.176	—.136	.000	.471	—.471	—.083	.697	—.780				— .956	.368	—1.324
		N=32			N=32			N=24			N=28			N=16	
M	.042	.162	—.119	.016	.438	—.422	—.109	.629	—.738	—1.027	.470	—1.497	—1.053	.367	—1.420

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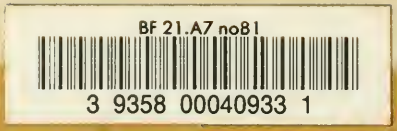
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